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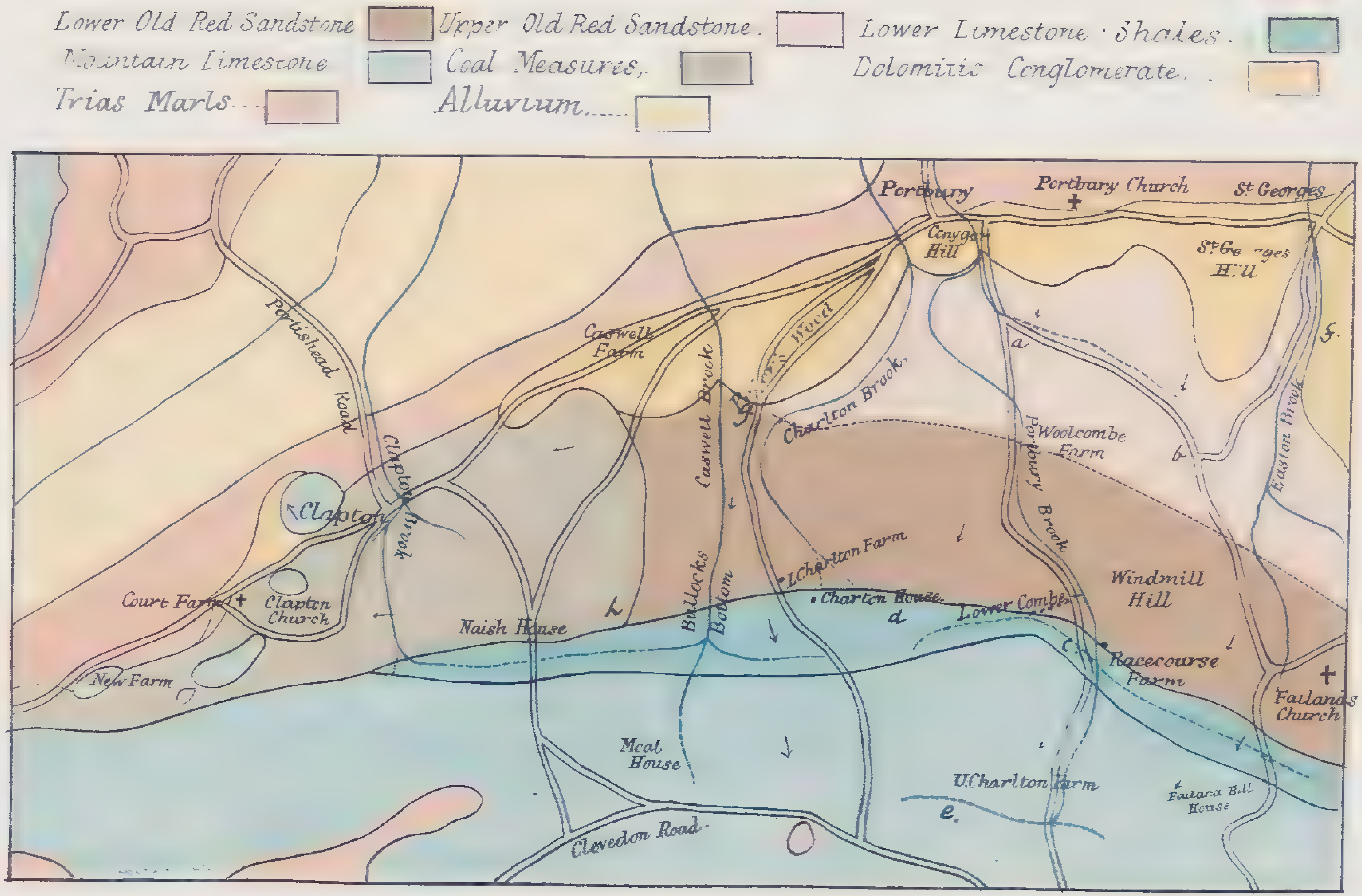


Plate I.

Map of The Portbury and Clapton District.

Scale 1.5 inch to 1 Mile.

Contributions to the Geology of the Avon Basin.

III.

THE PORTBURY AND CLAPTON DISTRICT.

BY PROF. C. LLOYD MORGAN, F.G.S., ASSOC. R.S.M.

CONTENTS.

1.—Introduction.
2.—The Physical Features of the District.
3.—The Geological Structure of the District.
4.—The Connection between Scenery and Structure.
5.—The Greater Clapton Fault.
6.—The Lesser Clapton Fault.
7.—Conclusion.

1.—*Introduction.*

IN a previous paper ("Sub-aerial Denudation and the Avon Gorge," Proc. B.N.S. vol. iv., pt. iii., p. 171), I recorded my intention of endeavouring to investigate in some detail the influence of geological structure on the scenery of the Avon Basin, and at the same time of contributing somewhat towards the verification of the recognised geological map of this area. I then proceeded to consider the section of country on both banks of the Avon, to the N.W. of the city of Bristol, the map appended extending a little to the

W. of St. George's, Easton. In the present contribution I propose to consider the country somewhat farther west, in the district of Portbury and Clapton.

Readers of my former paper will remember that N. of the limestone ridge of Leigh Downs, there runs a depression in the softer Lower Limestone Shales, which, with the slopes of the adjoining limestone, is the collecting ground of the head waters of the Somersetshire tributaries of the Avon. These tributaries cut notch-like ravines in the Failands ridge of hard conglomeratic Old Red Sandstone, and these themselves receive minor tributaries in the more open country, due to the incoming of the softer sandy beds of the Old Red Sandstone, after which they again enter small gorges of greater or less length, cut in the harder and more resisting Dolomitic Conglomerate, on emerging from which they fall into the Avon, or (in the case of the Easton brook) enter on alluvial ground. The general conclusion arrived at was that the Failands ridge and the rising ground occupied by the Dolomitic Conglomerate are outstanding features which have resisted the sandpaper action of the general or superficial denudation of rain and frost, but which have been cut into by the file-like action of the special or linear denudation of the streams. For it is as characteristic of general denudation that it acts unequally on rocks of different powers of resistance, as it is characteristic of linear denudation that its stream-cut notch is as deep in hard as in soft rocks, since for a river to cut deeper into the softer than into the harder strata would necessitate its *running uphill* to get over the harder ridge.

2.—*The Physical Features of the District.*

On the southern border of the area we are now to consider runs the elevated ridge which joins Clevedon and Clifton.

Along its northern border is low ground, but slightly raised above the sea level. Down the northerly slope which separates these two run several streams. To the E. is the Easton brook, mentioned in my former paper; then follow the Portbury brook, Charlton brook, and Caswell brook; while to the W. is the Clapton brook.* But the slope down which these streams flow is by no means uniform, nor are the valleys in which they run of similar character throughout their course.

If the reader will take train to Portbury, and then ascend Conygar Hill, which lies just N. of the Priory, he will find that he stands in an ancient camp on a detached portion of an irregular ridge running roughly speaking E. and W. On either side of the hill the ridge is broken by the Portbury brook to the E., and the Charlton brook to the W. The ridge moreover sends out strongs purs southwards. Immediately to the E. of Conygar Hill is a spur, beyond which the ridge is slightly notched by an old stream-course long ago deserted. Farther E. is the still bolder spur of St. George's Hill, beyond which is the Easton brook, bounded still farther E. by a still bolder spur. To the W. a wooded spur bounds the left bank of the Charlton brook.

South of the irregular ridge on which he stands, which we may call the Portbury ridge, lies an open valley, in which run the Portbury and Charlton brooks, the watershed or divide between which is very low, as is also, but not so low, the divide between the Portbury and Easton brooks.

To the S. of this open valley is a second irregular ridge, on which to the extreme E. stands the newly built Failands Church, near the source of the Easton brook, and in which, nearly due S. of Conygar Hill, the Portbury brook cuts a well-marked notch between Windmill Hill on the E. and the birch woods of the Charlton estate on the W.; while yet farther W. the Charlton brook cuts a second notch, less conspicuous from this point of view, but well-marked when the stream is followed up to this point of its course. To the W. of this stream the Failands and Portbury ridges coalesce in Priors Wood.

---

* I give these names for convenience of reference. They are not named on the Ordnance Survey map.

Let the observer now follow up the Portbury brook, taking the road to the E. of Conygar Hill, and turning to the right where it bifurcates. In a quarter of a mile or less the brook passes under the road, having been here diverted to the W., that a fall of water might be obtained for driving Portbury Mill, which stands at the foot of Conygar Hill. Above this point the road follows the course of the stream, entering and passing through the wooded combe before mentioned, towards the upper end of which water for the supply of Portishead is taken from the brook. A side road to the right, leading up to a farm (Lower Combe), runs in a line of depression recently occupied by a tributary streamlet, but now dry. Followed up, this old stream-course leads us into the broad and open depression which lies to the S. of Charlton House. Returning to the main streamlet, we find that somewhat farther up the road the valley opens out, especially to the left, where a well-marked line of depression runs in a S.E. direction. The depression to the N.W. is less marked. After this the road follows the now insignificant stream to its source in a spring at the foot of the main ridge, which lies to the S. of our area. Although the stream course here ceases, the line of depression is carried on by a combe, near which the road runs. This runs for a few hundred yards in a southerly direction, and then turns off sharp to the W., and leads into a basin-like depression near Upper Charlton Farm.

So that the Portbury brook, rising in the southern ridge (part of that which runs from Clifton to Clevedon), crosses a line of depression, cuts through a belt of high ground (continuous with the Failands ridge mentioned in my previous paper), enters and flows through the open country S. of Conygar Hill, cuts a notch in the Portbury ridge to the E. of that hill, and then escapes into the alluvial flats.

The streams on either side of it, the Easton brook and the Charlton brook, differ in that they rise in the second or Failands ridge, the latter cutting a deep combe in the ridge.

The Caswell brook rises near Moat House Farm, in the Clifton-Clevedon ridge. It too, like the Portbury brook, soon crosses a line of depression, where it receives tributaries larger and more important than itself. It then enters Bullock's Bottom, the beginning of a long stream-cut combe, from which it emerges near Caswell Cross cottages. In this combe, however, one misses the open space which, in the case of the Portbury brook, separates the upper notch in the Failands ridge from the lower notch in the Portbury ridge; for the Failands and Portbury ridges here coalesce.

To sum up then the features of the Portbury district: To the S. lies the Clifton-Clevedon ridge, in which rise the Portbury and Caswell Brooks; N. of this is a narrow line of depression; farther N. is a ridge of irregular width, which is notched by the Portbury and Caswell Brooks, and in which rise the Easton and Charlton Brooks; still further N. is the open valley, over which the Camp on Conygar Hill keeps guard, and which narrows and ceases to the W., owing to the coalescence of the two ridges that bound it. Along its northern border is the irregular Portbury ridge, notched by all four brooks; beyond which again to the N. is the low ground stretching to the Severn.

In the adjoining district of Clapton the physical features are somewhat different. The line of depression N. of the Clifton-Clevedon ridge extends a little to the W. of Naish House, but beyond this has no existence. N. of this depression the country slopes tolerably uniformly down to the low-lying alluvial ground. The Clapton brook rises to the extreme W. of the depression, near Naish House, and then flows in a wooded combe to more open country, where it receives small tributaries on either side before reaching the alluvium of the moor.

But perhaps the most noticeable features in the western part of the Clapton district are the rounded, often tree-covered, bosses on the slope. One of these is seen just to the N. of the village; another to the left-hand side of the road leading to the church, and a third above Court Farm.

Altogether, the physical features of the neighbourhood of Clapton differ somewhat markedly from those of the Portbury and Failands districts which lies farther E.

3.—*The Geological Structure of the District.*

It requires but little acquaintance with the rocks of the neighbourhood of Bristol to enable the observer to ascertain that Conygar Hill is composed of Dolomitic Conglomerate. The beds are shown in natural outcrop on the hill, and are well exposed in the large quarry on its W. side. They are seen to dip gently towards the Severn. The size of the fragments is variable, being largest in the lower part of the S. end of the quarry, where they are perhaps not far from their junction with the Old Red Sandstone. From this point they diminish in size: (1) as we rise into overlying beds; (2) as we pass northwards (which was in Triassic times, seawards or lakewards). The large included fragments are angular, or but little rounded, and many of them derived from the Mountain Limestone, some from the Old

Red, while I have detected a few fragments of the Bryozoa Bed of the Lower Limestone Shales. It is difficult to understand how this deposit, containing so large and such mixed fragments, angular or but little rounded, and many of them from some little distance, embedded moreover in a matrix which also contains smaller also sub-angular fragments, could have been formed except by the agency of ice.

The hill is entirely composed of Dolomitic Conglomerate; and it is unfortunate that on Mr. Sanders' map the name *Conygar Hill* is printed, not on the hill itself, but on the Old Red Sandstone which lies to the S. The whole of this irregular ridge, of which this hill forms a fragment separated by the notches cut by the Portbury and Charlton brooks, is composed of this resisting rock, a belt of softer low-lying Triassic Marls forming a band between this rock and the alluvium of the flats.

The open country S. of Conygar Hill is composed of soft sandy beds of Old Red Sandstone, the loose and friable nature of which is well seen at the points marked *a* and *b* on the map. The upper beds of Old Red Sandstone, the outcrop of which is farther N., are, in fact, conglomeratic, and for the most part harder and of greater resisting power. It is these upper beds which give rise to the ridge through which the Portbury brook cuts the wooded combe to the W. of Windmill Hill. The line of depression to the N. of this ridge marks the outcrop of the Lower Limestone Shales, but the now dry tributary which once flowed through Lower Combe, cut across the Old Red Sandstone, as shown by the dotted lines on the map.

Let me here turn aside for a paragraph to note a matter of detail, but one of some little importance.

The student of our local geology should not fail to make himself acquainted with one of the most characteristic beds of the Lower Limestone Shales, the Bryozoa Bed. It marks a definite horizon in this series, and in the Avon section will be found in the cutting below Cook's Folly, about 120 feet above that quartzose conglomerate of the Old Red Sandstone which Mr. Stoddart takes as 33 feet below the base of the Lower Limestone Shales. It is, therefore, about 90 feet from the base of the Shales. It is a coarse reddish Limestone, in which are embedded enormous numbers of fragments of Polyzoa, Encrinital fragments, and other organic remains, composed of deep red oxide of iron. On treating the rock with dilute hydrochloric acid, the calcareous matter dissolves, and these polyzoan and other fragments remain behind as an insoluble residue. I have been able to detect this bed at two places in the area under consideration—at the point marked *c* in the road opposite Racecourse Farm, and at the point marked *d* in a quarry to the E. of Charlton House.

North of the line of depression caused by the Lower Limestone Shales is the Clifton-Clevedon ridge of solid Mountain Limestone. The average dip of the beds is here about 25° S.S.E. But near the Upper Charlton Farm the beds are nearly vertical, with an E. and W. strike. The rock has been quarried here for lime, and the beds exposed (at *e* on the map) very closely resemble those at the foot of the gully near the Sea-wall; so that the line of depression here may be due to the incoming of the beds that in my previous paper (*Avon Gorge*, p. 190) I called the Middle Limestone Shales. The high dip must be due to a local roll or dislocation of the strata.

Setting aside such local disturbances, the general direction and angle of dip of the Palæozoic beds, Old Red Sandstone, Lower Limestone Shales, and Mountain Limestone, is from 15° to 30° S.S.E., the Dolomitic Conglomerate and Triassic marls lying on the upturned edges of the lower beds of the Old Red Sandstone.

The junction of the two series is well seen at the point marked *f* on the map, where the Easton Brook has cut down through the Dolomitic Conglomerate into the Old Red Sandstone. The Dolomitic Conglomerate is here of the characteristic largely-fragmental texture. The junction may also be seen under different conditions at the point marked *g*, where the new road just constructed on the Charlton estate joins the older and steeper road. Here the somewhat decomposed Dolomitic Conglomerate reposes on the upturned edges of the ancient gravel beds of the Old Red Sandstone. These ancient gravel beds are well seen in a quarry about 100 yards farther along the road towards Charlton House. They resemble those exposed on the shore line of the Portbury-Clevedon ridge, W. of Woodhill Bay. A better example of a consolidated gravel of ancient date could scarcely be found.

The reason for the coalescence of the Portbury and Failands ridges is now evident. The Dolomitic Conglomerate which forms the former here lies on the harder beds of the Old Red Sandstone which give rise to the latter. The softer beds have not been exposed to the modern processes of denudation. It was, indeed, eaten into by pre-Mesozoic denudation; but over the eroded edges the Dolomitic Conglomerate was deposited. The Dolomitic Conglomerate ridge marks the old shore line of the Triassic sea or lake. At Conygar Hill the softer Old Red beds had probably not been so far denuded; and over them no protecting layer of Dolomitic Conglomerate was laid down. But farther W. the pre-Mesozoic waves had planed down the lower and softer beds, and played upon the harder ridge, against which the Dolomitic Conglomerate was thus deposited.

Somewhat west of the Charlton brook, beds of a very different character make their appearance. N. of the Lower Limestone Shales, near Naish House, we find, instead of Old Red Sandstone, Coal Measures. Farther W. the Coal Measures seem to be in contact with Mountain Limestone; the band of Lower Limestone Shales ending off as shown on the map.

The nature of the Coal Measures (the dip of which varies both in direction and angle, though the general tendency is W.N.W.) will be gathered from the following section from a boring near Court Farm:—

Soil and New Red Sandstone	10	feet.
Dolomitic Conglomerate	11	,,
Pennant	114	,,
Bed of Marl, with Water	1	,,
Pennant	20	,,
Clay, with 10 inches of Coal	6	,,
Shales	60	,,
Pennant	220	,,
Shales	5	,,
Pennant	63	,,
	510	,,

Anstie (*Coal Fields of Gloucestershire and Somersetshire*, p. 55), from whom I quote this section, remarks: "This shows a thickness of Pennant Grit nearly if not quite as great as that at Nailsea, and hence it is possible that the lower coals may be at no great depth below."

Finally, the rounded bosses on the slope of the Clapton district disclose themselves on examination to be Mountain Limestone, of which our survey memoir says: "Around Clapton-in-Gordano are several patches of Carboniferous Limestone occurring in the midst of the Coal Measures, positions which it seems difficult to account for" (p. 21).

4.—*The Connection between Scenery and Structure.*

There is to my mind no doubt that the scenery of the district is the direct product of the action of denudation. The streams, with their file-like action, have cut down valley-notches to the same depths in harder and softer rocks; but the general or superficial denudation, due to rain, frost, and weather, which I before likened to the action of sandpaper, has broadened out the valleys and lowered the water-sheds where the strata are soft, and has left the harder materials outstanding as ridges, notched here and there by the brook-files.

Taking the Portbury district, the strata stand as follows with regard to their resistance to the action of superficial denudation:—

Mountain Limestone	*resisting.*
Lower Limestone Shales	*yielding.*
Upper Old Red Sandstone	*resisting.*
Lower ,, ,,	*yielding.*
Dolomitic Conglomerate	*resisting.*
Trias Marls	*yielding.*

And throughout the district the yielding beds are low-lying or occupy depressions, while the more resisting beds stand out as ridges.

As we shall see in the next section, a great fault separates the Coal Measures of the Clapton District from the Old Red Sandstone of the Portbury district. One might well suppose that a fault with a throw of some thousands of feet would have left a well-defined mark on the scenery of the area; but it is not so. A change of scenery is indeed produced; but this is due to the fact that beds of different power and mode of resistance are introduced. It is a noteworthy fact that the exact line of fault between Coal Measures and Old Red Sandstone has to be somewhat arbitrarily filled in on the map, the only indication of its probable position on the surface being a line of depression above a spring a little to the west of Caswell Farm, together with that afforded by small springs and damp ground; and this notwithstanding the fact that a difference of geological level of some thousands of feet has been brought about by the fault.

The rounded knobs of Mountain Limestone in the midst of the Clapton Coal Measures stand out by reason of their resisting power. They are, I believe, isolated fragments of a once continuous limestone sheet.

5.—*The Greater Clapton Fault.*

The introduction of Coal Measures in the Clapton district can only be explained by a great fault which runs just to the N. of the Clifton-Clevedon ridge in a W.S.W. and E.N.E. direction. About a quarter of a mile E. of Naish House, it is either met nearly at right angles by another fault running nearly N. and S., or more probably here makes a sharp curve northwards. Nowhere, however, so far as I know, can the actual junction of the faulted rocks be seen.

In a plan and section of the Clapton Coal Field by Mr. David Llewellin, kindly forwarded me by Mr. Davis, of Portishead, this fault is very imperfectly indicated with a throw of not more than two or three hundred feet. In another point, too, the diagram is unsatisfactory, and, I think, misleading. It shows the Carboniferous Limestone at a short distance (50 or 60 yards) below the coal seams, of which two are marked; this gives what I believe to be a very erroneous impression. There can scarcely be a doubt that beneath the Pennant beds in which the boring was carried, *there lies the whole series of lower Coal Measures*, probably of a thickness of from 12 to 15 hundred feet.

Let us try and estimate the throw of the fault. At the

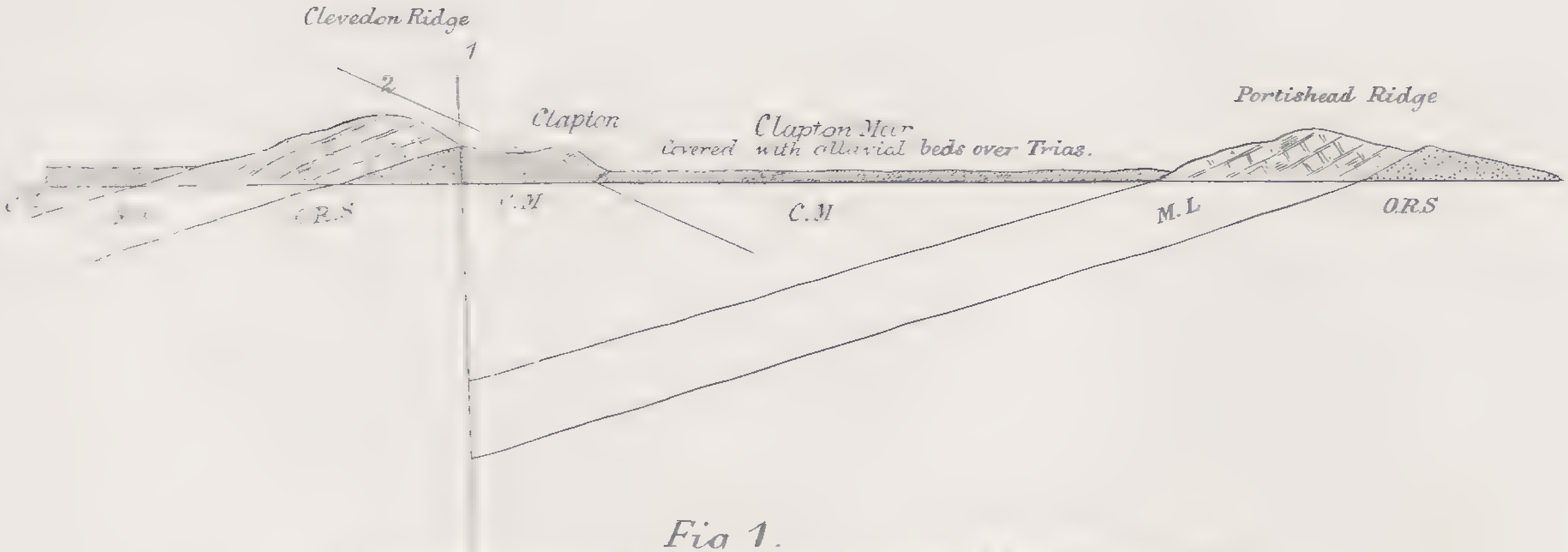


Fig 1.

Diagram Section to show the Greater 1) and Lesser(2) Clapton Faults.

Triassic and alluvial beds omitted)

point marked *h* on the map, Coal Measures, presumably Pennant Sandstone, are thrown into juxtaposition with Old Red Sandstone. Taking the dip of the beds (about 20° S.S.E.) into consideration, the beds of the Old Red Sandstone at this point are certainly not less than 700 feet from the top of that series. Supposing that we have here the lowermost beds of the Pennant brought down to the level of these Old Red beds, then we have intermediate to these two—

Lower Coal Measures	1500	feet.
Millstone Grit	500	,,
Upper Limestone Shales	500	,,
Mountain Limestone	1500	,,
Lower Limestone Shales	300	,,
Old Red Sandstone	700	,,
	5000	,,

It is possible, however, that the Millstone Grit is not developed here. It is said not to be developed at Tickenham, on the Nailsea side of the Clifton-Clevedon ridge, and it is not mapped by the Survey or Mr. Sanders near Clevedon, where we should expect it to occur. Striking out this 500 feet, therefore, and striking out another 500 feet for not impossible thinning of some of the other beds, we still have 4000 feet separating the beds to the W. of *h* from the beds to the E. of that point.

In another way the magnitude of the displacement may be seen and a rough estimate of its amount may be made. The Mountain Limestone being here thrown down far beneath the surface of the ground, and the beds dipping to the S. S. E., this rock should emerge to the N. N. W. And in the Portishead-Clevedon ridge we see that it actually does so. Now the emergence of the point of junction of the Lower Limestone Shales and the Old Red Sandstone is distant 2⅓ miles, or say 12,000 feet N. N. W. from the Clifton-Clevedon ridge at the point where the fault curves off to the N. The average angle of dip may be taken at 20°. With these data, viz., the distance and the angle of dip, we can estimate approximately the amount of vertical displacement of the strata. It is 4,104 feet; and since the average angle of dip is probably greater than 20°, we may, I think, take 4,000 feet as a minimum estimate of the throw of the fault at the eastern end. At the same time I should mention that the state of disturbance of the strata in the neighbourhood of Portishead leads me to place very little reliance on this mode of estimating the amount of vertical displacement.

As we pass westwards towards Clevedon, the throw becomes less and less. This is clearly seen by the gradual approximation of the Portishead-Clevedon ridge and the Clifton-Clevedon ridge. At Clevedon the two ridges meet; the fault has so nearly died out westwards as not to be sufficient in amount to separate limestone from limestone.

6.—*The Lesser Clapton Fault.*

The existence of bosses of Mountain Limestone in the midst of the Coal Measures at Clapton is at first sight not a little strange and puzzling. I make use of the 6-inch ordnance map on which to note my detailed geological observations; and I find that my "M. L. 25° S. W.," recording an observed dip of Mountain Limestone, is wedged tolerably closely in between two notes, "Old Shaft (Coal)," printed on the map. So, too, in the road leading from Clapton to Clapton Church, there is clear evidence of coal in the bank; while at a higher level, and within 60 yards, is Mountain Limestone quietly dipping at a gentle angle (14°) towards the moor!

Puzzling, however, as this Mountain Limestone in the midst of or resting on Coal Measures at first sight appears, it may, I think, be explained.

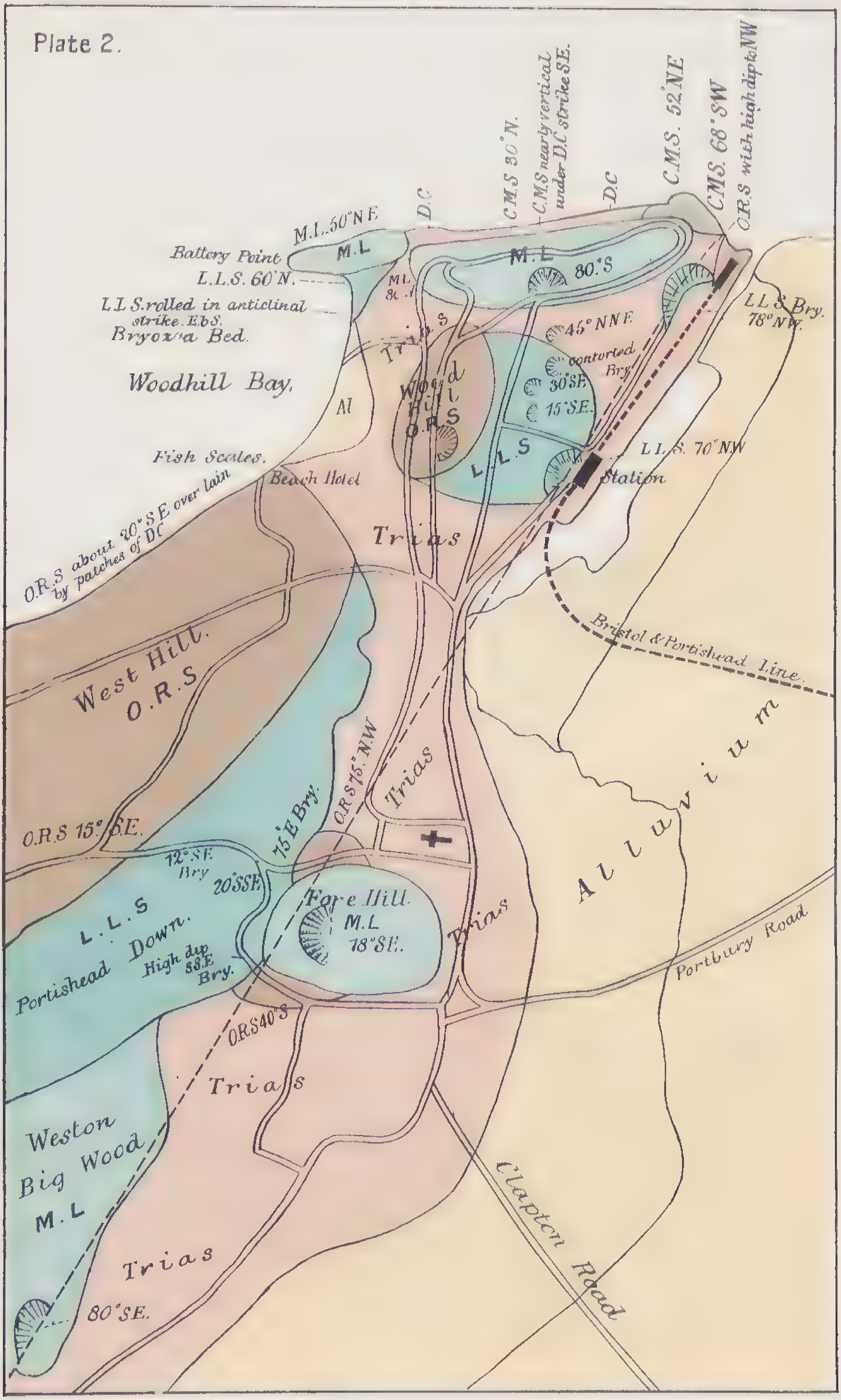
My hypothesis is, that subsequent to the Greater Clapton Fault there occurred a minor flat-lying fault, with a down throw towards the present valley, that is N. N. W. By this means, as shown in the diagrammatic section, Fig. 1, Mountain Limestone was shifted downwards and northwards in such a way as to come to overlie the Coal Measures brought down by the greater fault. The somewhat variable dip of the faulted limestone is only to be expected in the neighbourhood of such a dislocation. Of the mass of limestone thus faulted down, only isolated patches now remain—detached fragments separated from each other by subsequent denudation.

7.—*Conclusion.*

I venture to think that the facts brought forward in this paper will serve to confirm that which it is one of my objects to enforce—that the character of our scenery is determined by the geological structure of the beautiful country that surrounds us. It is my part, as geologist, to point out how what I may term the naked scenery—the scenery unclothed with the rich vesture of vegetation—is due to the action of the forces of denudation. But when my task is done, that of the botanist (and the pages of our Proceedings show that we have good botanists among us) begins. He completes the work that we of the geological section begin. He shows how the rich green vesture, no less than the bare surface that it so beautifully invests, is determined by the nature of the soil, the character of which is in turn the direct outcome of the character of the strata that give rise to it. And since the distribution of animal life is to a very large extent determined by the distribution of vegetal life, this too is ultimately determined in no slight degree by geological structure.

Not to go beyond the bare elements of scenery, however, with which alone I am here directly concerned, let me remind the observer of this fact, that the physical features of our district are partly produced, *and partly reproduced*, by the denudation at present in progress. The Clifton-Clevedon ridge, the Portishead-Clevedon ridge, like the "Westbury Horseshoe," Backwell Down, and the Mendips, are old scenic features which were in existence, perhaps more markedly than now, in pre-Mesozoic times. Upon the upturned and denuded edges of the strata of which they are formed were laid down the Dolomitic Conglomerate, Trias marl, Lias, and Oolitic rocks. It has been the work of latter-day denudation to remove to a very large extent this thick Mesozoic cover, and having done so to proceed to refashion and carve afresh the old features produced by the pre-Mesozoic denudation. How far such physical features as the Lower Limestone Shales depression and the soft and open valleys of the lower beds of Old Red Sandstone are the product of modern denudation, or the survival of the denudation of a by-gone age, it is impossible to say for certain. But it is my own belief that these features are modern modifications of the old rough-hewn model, and are, in their present form, the outcome of the sandpaper action of modern frost and rain, and the file-like action of the present brooks and streams.

Plate 2.



Sketch Map of Portishead

O.R.S. Old Red Sandstone. ▭

L.L.S. Lower Limestone Shales. ▭

D.C. Dolomitic Conglomerate.

M.L Mountain Limstone. ▭

C.M.S. Coal Measure Sandstone. ▭

Trias ▭ Alluvium ▭

Scale, 3 in to 1 Mile.

Contributions to the Geology of the Avon Basin.

IV.

ON THE GEOLOGY OF PORTISHEAD.

By Prof. C. LLOYD MORGAN, F.G.S., Assoc. R.S.M.

CONTENTS.

1.—Introduction.
2.—Physical Features and General Geological Structure.
3.—Flexures and Faults.
4.—Conclusion.

1.—*Introduction.*

TO those who would understand the scenery of our neighbourhood, and would trace its connection with the geological structure which underlies it, Portishead is a locality of interest. Little has been written, however, on the geology of Portishead. In Buckland and Conybeare's classical *Observations on the South-western Coal District of England* (*Trans. Geol. Soc.*, 2nd series, vol. i., p. 210) the district is incidentally considered, and views are put forward to which attention will be drawn in the sequel. In Sir Henry de la Beche's Memoir *On the Formation of the Rocks of South Wales and South-western England* (*Mem. Geol.*

Surv., vol. i.) a section is given of the Dolomitic Conglomerate. A section of the Old Red Sandstone to the south of Battery Point will be found in the pages of our own *Proceedings* (vol. ii., No. 8, p. 79); and the occurrence of fish remains in these beds has been noted (Baily, *Brit. Assoc.*, 1864, p. 49, and *Proc. B.N.S. loc. cit.*). In his second paper *On the Geology of the Bristol Coalfield*, Mr. W. W. Stoddart gives a section, reproduced in Fig. 2. Finally, in a little volume on *The Natural History of Portishead*, by J. N. Duck, there is a geological map of the district; and a few pages are devoted to a short and for the time (1852) conventionally convulsional account of the district and the dislocations to which it has been subject. This seems to be the sum of the geological literature of the locality.

In our recognised geological maps, that by Mr. William Sanders, and that by the Geological Survey, there is much valuable information, with some differences of detail. Such maps, however, are, taken by themselves, insufficient to indicate the varying strike and dip of the rocks and the not altogether unimportant problems which suggest themselves when the beds are practically examined in the field.

I venture therefore in this paper to set down certain observations, and to draw attention to, if I cannot solve, some of the suggested problems.

2.—*Physical Features and General Geological Structure.*

Portishead lies at the north-easterly end of a well-marked ridge, which runs in a south-westerly direction to Clevedon. As was clearly pointed out by Buckland and Conybeare, the ridge "is divided along its summit, by a longitudinal valley, into two parallel crests, the southern consisting of Mountain Limestone, the northern of Old Red Sandstone, an intermediate valley marking the Shale that divides the two formations" (*loc. cit.*, p. 239). Near Portishead, however, the symmetry of the ridge is somewhat broken ere it ends abruptly on the shore of the Severn.

If the reader of this paper would make himself practically acquainted with the district of which it treats, let him take train to Portishead. As he leaves the station he will see on the other side of the road an exposure of some of the beds of the Lower Limestone Shales, dipping at a high angle to the N.W. Turning to the right, and proceeding towards the landing-place, he will reach in a few minutes a considerable quarry, in which beds of the same series, also dipping steeply N.W., but curving over near the surface, are being worked for the embankment just outside the station. The position of these beds is fixed by the occurrence of the Bryozoa Bed (see Cont. 3, p. 8), of which characteristic specimens may here be readily obtained. The position of this bed in the Avon section is, it will be remembered, about ninety feet from the base of the series of Lower Limestone Shales.

To the N.E. of the quarry is the station near the landing stage. Here beds of Old Red Sandstone are seen. At first sight they seem to have a gentle dip to the N.E.; but more careful investigation shows that this appearance is deceptive, and that the beds in truth dip steeply to the N.W.

Climbing down on to the rocks beneath the hotel grounds, they are found to be composed of a reddish sandstone overlain by Dolomitic Conglomerate. The grain of the sandstone, however, is not like that of the Old Red, and the tolerably abundant occurrence of indistinct but unmistakable plant remains of a Carboniferous type shows that the sandstone belongs to the Coal Measure series. The beds dip steeply in opposite directions, *viz.* N.E. and S.W., within a space of 100 yards, but the strike remains the same.

Ascending to the road through the hotel grounds, we find ourselves on a wooded ridge of Mountain Limestone, which runs in a westerly direction to Battery Point, and the eastern end of which is surmounted by an ancient camp. The Mountain Limestone is nearly vertical, varying in the main ridge from 10° to 15° on either side of the perpendicular. Its northern or channel side is fringed with Dolomitic Conglomerate, beneath which here and there Coal Measure Sandstone is exposed to view. It is separated on its southern flank from the Old Red Sandstone of Wood Hill by a line of depression also filled with Triassic beds, beneath which in all probability lie Lower Limestone Shales. A little band of Trias seems to separate the main ridge from the smaller ridge on which the battery stands. The beds at Battery Point are found to dip at a less steep angle of 50° to 60° to the N.E. They are full of Crinoidal fragments with some Spirifers, and would seem to belong to the base of the Mountain Limestone series.

Skirting the shore of Wood Hill Bay, we soon come to an exposure of Lower Limestone Shales, where the beds are folded into quickly succeeding curves, the summits of the anticlinals being eaten into by denudation. To the south of this exposure the Bryozoa Bed is again displayed. The strike of the folded beds is E. by S.

Still continuing to skirt the bay, with the isolated knoll of Wood Hill to our left (E.), we cross a triangular patch of alluvial ground, between which and the Severn muds is a beach-ridge of Old Red pebbles. Near the charred remains of the Beach Hotel we find the Old Red Sandstone well exposed. The strata show strong conglomeratic beds, with pebbles of opaque white quartz; sandy beds, with excellent examples of false bedding; and, where the wearing back of the cliff has exposed the surface of the beds, abundant evidence of so called ripple marks. It was at this point that

Mr. Baily and Dr. Martyn found Holoptychius scales. A short search on three or four occasions has only rewarded me with one small fragment. It was at this point, too, that the section recorded by the Naturalists' Society was made. The beds dip gently at an angle of somewhat less than 20° S.S.E.

From this point we may take the road which ascends West Hill. In doing so we rise on to the more northerly of the parallel crests spoken of by Buckland and Conybeare. From this point the crest may be followed without serious break to Clevedon. Between it and the Old Red Sandstone of Wood Hill, however, which lies in the same line, there is low-lying ground, occupied by Triassic beds. Following the road to the corner where O.R.S. 15° S.E. is recorded on the map, we turn to the left, and, proceeding eastwards, descend on to the Lower Limestone Shales, which are, however, only very imperfectly shown at one or two points by the roadside, at one of which, however, the Bryozoa bed is exposed.

On reaching the place where the road forks, and taking the more easterly (left hand) branch, we descend into a lane, near the top of which beds, proved to be Lower Limestone Shales by the occurrence of the Bryozoa bed, are seen in a nearly vertical position, one bed giving an E. dip of 75°, others perpendicular, while a marly bed shows contortions. A little farther down this lane beds of unmistakable Old Red Sandstone put in an appearance, while still farther down, near a cottage, characteristic conglomeratic beds, with pebbles of opaque white quartz, are seen, the beds here dipping steeply about 75° N.W. A little lower down they are overlain and hidden from view by Triassic beds.

Returning to the place where the road forks, and taking the other (right-hand or southerly) branch, we descend another lane, in which near the top are obscurely seen

Limestone beds, probably Lower Limestone Shales, near the base of the Mountain Limestone, dipping 20° S.S.E. Farther down the lane more silicious beds cross the road with a higher dip, while at the point marked on the map (Bry.), I found on both sides of the road evidences of the Bryozoa bed. Yet farther down the lane we come upon a good exposure of conglomeratic Old Red, with pebbles of opaque white quartz, dipping 40° S.

After thus finding characteristic Old Red at the lower end of both these lanes, which will be seen from the map to lie one on either side of Fore Hill, it is somewhat surprising to find that the hill itself is composed of Mountain Limestone. The beds are very well exposed in a large quarry, which was opened out when the Portishead Docks were in process of construction. They are characterized by the occurrence of curious bands and nodules of white Chert, which has in places replaced the Limestone, converting the organic remains imbedded therein (mostly crinoidal "ossicles," with some Spirifers) into hard Silica. The beds dips at a uniform angle of about 18° S.E.

Standing upon Fore Hill, we see to the S.W., clothed by Weston Big Wood, the eastern termination of the more southerly crest mentioned by Buckland and Conybeare, of which the Limestone knoll upon which we stand seems like an outlier. On this view, however, the steeply inclined Old Red Sandstone in the lanes on either side of the hill seems not a little puzzling.

Before returning to the station we may visit, first, a large quarry in Wood Hill, where the Old Red Sandstone is well seen, dipping about 25° E.S.E., and then four or five smaller quarries to the west of it, on the slope above the station, where beds of Lower Limestone Shales are exposed, with changing direction and angle of dip. In the more southerly quarries the beds dip gently 15° S.E. In one of the more northerly they are contorted, and then seem to curve round so as to dip 45° N.N.E. In two of these quarries we once more come upon the Bryozoa bed.

The occurrence of this remarkable bed in the Avon section was first indicated by Mr. W. W. Stoddart, who described it and some of its contents in *The Annals and Magazine of Natural History*, vol. vii. (1861), p. 486. It consists of a reddish limestone, in which are imbedded a vast number of fossils of minute organisms, chiefly crinoidal remains, with polyzoa fragments. The following is Mr. Stoddart's list of forms always present in the rock beneath Cook's Folly: "*Ceriopora rhombifera* (Goldf.); *Platycrinus lævis* (Mill); *Poteriocrinus isacobus?* (Aust.); *Leperditia Okeni* (Munst.); *Cypridina ovalis* (Stod.); *Cytherella lunata* (Stod.); *Naticopsis plicistria* (McCoy), (Young); *Productus* (Sp.?); *Spirorbis triangulatus* (Stod.); *Psammodus porosus* (Ag.); *Cladodus conicus* (Ag.)." Mr. Stoddart's list I quote as it stands (*Proc. B. N. S.*, New Series, vol. i., p. 320), without criticism or addition. The organic remains are exquisitely preserved. On treatment of the rock with dilute acid the limestone dissolves, and the microzoa remain as a deep red ferruginous residue.

So far as I am aware, the Bryozoa bed has not been recorded elsewhere than in the Avon section. In the map appended to this paper I have recorded six other localities, and in that appended to my paper on the Portbury and Clapton District (Con. 3), two more. Where I have been able to measure it, the thickness has been from 5 to 8 feet. The size and number of the contained microzoal fragments varies, but the essential character is remarkably constant. In all cases it is a reddish limestone, in which are imbedded the insoluble ferruginous fossils.

Mr. H. M. Smith has been good enough to make for me, in Professor Ramsay's laboratory at the University College, Bristol, chemical analyses of two specimens of this rock, (1) from the Avon Section, (2) from the large quarry near the landing-stage at Portishead.

		1.	2.
Matter soluble in dilute acid, mostly calcium carbonate		66·46.	68·08
Matter insoluble	Silica	9·31.	5·16
	Ferric Oxide	24·23.	26·76
		100·00	100·00

Microscopic examination of the insoluble residue seems to indicate that the Silica is free as Quartz, and that the Microzoa have been converted into Peroxide of Iron.

Before passing on to consider the flexures and faults of which we have evidence at Portishead, it will be well to summarise the physical features of the district. This is readily done. To the S.W. West Hill and Weston Wood form, respectively, the terminations of the northern and southern crests of the Portishead-Clevedon ridge. Of the Limestone crest, Fore Hill appears to be an outlying portion, separated by a valley partly occupied by Trias. But perhaps the general trend of the southern ridge would lead one to expect that this outlier should be farther south. Of the Old Red Sandstone crest, Wood Hill appears to be an analogous outlier, separated by a low gap, occupied by Triassic beds. Farther north, instead of anything like a continuation of these longitudinal crests, we have a ridge which lies obliquely to them, composed of Mountain Limestone, and overlooking the Severn.

A summary of the geological structure will find a place more fitly at the beginning of the next section.

3.—*Flexures and Faults.*

The essential features to be noted in the geological structure seem to be the following:

1. The normal strata of the Portishead-Clevedon ridge thrown into their present position by the greater Clapton Fault (*see* Portbury and Clapton District, p. 12). The beds dip gently at an angle of 30° or less S.S.E. They may be seen on the coast-line S.W. of Woodhill Bay, and in the large quarry at Wood Hill.

2. Along a line to the S.E. of this, marked by a dotted line on the map, the strata are highly inclined. They dip 78° N.W. in the large quarry near the N. end of the line, 70° N.W. near the station, 75° N.W. in the lane to the N. of Fore Hill, 40° S. in the lane to the south of that hill, and 80° S.E. in a quarry to the south of the Mountain Limestone of Weston Wood.

3. This line, however, crosses Fore Hill, in which the Mountain Limestone dips gently 18° S.E.

4. North of Wood Hill is a depression occupied by Dolomitic Conglomerate. On the coast, however, and in the quarry to the west of the hill, Lower Limestone Shales are seen contorted, and appear to mark the axis of an anticlinal.

5. North of this depression is the Mountain Limestone ridge, running from Portishead (Battery) Point to the landing-stage. The beds are nearly vertical, but near Battery Point dip about 50° N.E.

6. North-east of this ridge, just appearing on the coast, and overlain to a large extent by Dolomitic Conglomerate, are beds of red Coal Measure Sandstone. With a N.W. to S.E. strike they dip at high angles N.E. and S.W.

7. Overlying Triassic beds, which do not here call for particular notice.

In Buckland and Conybeare's paper before cited (p. 246), the limestone ridge between Battery Point and the landing-stage is regarded as belonging to a totally different limestone axis to that which forms part of the Portishead-Clevedon ridge; viz., to the axis of King's Weston Down and Penpole Point, on the Gloucestershire side of the Avon. "From Penpole Point," they say, "the limestone ridge points exactly to Portishead Point, on the south of the confluence of the Avon with the Severn; and on examination, the strata at that point are found to consist of Mountain Limestone mantling round the central nucleus of Old Red Sandstone. The limestone may be traced from the northern extremity of Portishead (Battery) Point, ranging in a southerly (?—see map) direction along a woody hill, to the old battery commanding Portishead (Woodhill) Bay. In the north-eastern angle of the bay the lower strata of the limestone, here approaching to the fault of the Clapton coalfield, are bent into zig-zag curves. On the south-east the Old Red Sandstone emerges from beneath the limestone, and forms an almost insulated hill (Wood Hill) between the village of Portishead and the limestone. On the north-west, along the coast, particularly near to Portishead Point, a covering of Dolomitic Conglomerate greatly conceals the inclined strata, which are displayed at intervals only, in low cliffs. At low water, however, we discover the Pennant coal-grit reposing against the calcareous chain, and may trace it nearly all the way from Portishead Point to the fort; and in the cliffs of a small cove, half way between the northern cape and the fort, the crop of a coal-bed is seen, having a roof of the Pennant abounding in vegetable remains. These strata are highly elevated, and occasionally vertical or even reversed in their dip. These coal measures seem to indicate the existence of a yet unexplored coal-basin in the valley of the Severn, between the calcareous chain now treated of, and that of Monmouthshire. The limestone rock, called the Denny, in the mid-channel opposite the mouth of the Avon, is situated exactly in the line which would connect the two calcareous chains. The strata, however, are nearly parallel to those at Portishead, dipping N.W. at an angle of 60°."

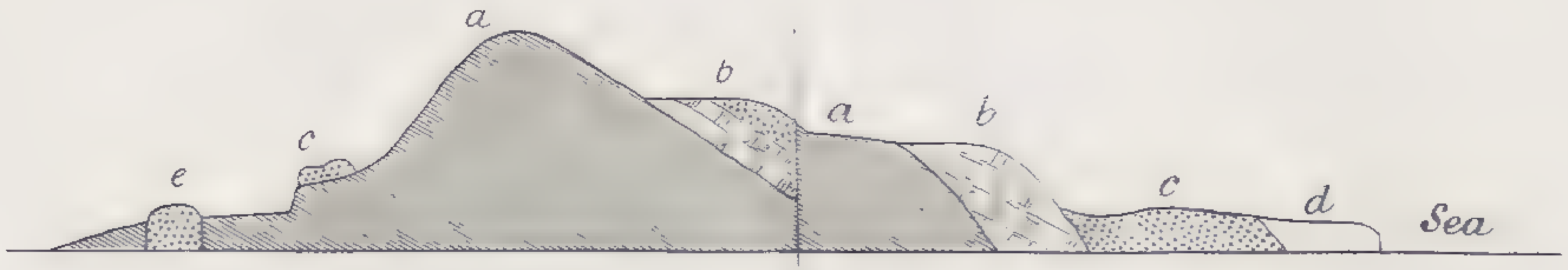
In the map appended to the paper from which I quote, the Greater Clapton Fault is laid down as passing between Wood Hill and West Hill. It would thus be continuous with the line which separates Old Red Sandstone and Coal Measures in the map which accompanies my paper on the Portbury and Clapton district. On the other hand, in the "Section from Portishead to Pamborough Hill" (Pl. xxxii. Sec. 3), the Pennant Grit is made to repose upon the Mountain Limestone without the intervention of a fault.

This I am persuaded is an incorrect interpretation of the facts. I regard Denny Island, and not the Portishead ridge, as the continuation of the King's Weston and Penpole axis. I can find no evidence of a line of fault between West Hill and Wood Hill, the latter hill being, I feel sure, an outlier of the more northerly crest of the Portishead-Clevedon ridge; in support of which I would point to the similarity of trend and the similarity of dip. The limestone ridge to the north is in connection with the Portishead-Clevedon axis, while I hold that the Coal Measure ("Pennant") Grit cannot have assumed its position on the coast without the intervention of a fault. That this fault has a throw of no small amount is shown by the fact that by it Old Red Sandstone, with high dip to the N.W., is thrown into close juxtaposition with Coal Measure Sanstone, probably Pennant, dipping 68° S.W.

Dealing now with the strata south of this fault, the connection of the groups numbered above 1, 2, 4, and 5, would seem to be as follows. Group 1, of strata dipping gently S.S.E., are the normal strata, but these along the dotted line in the map are locally folded, there being probably a synclinal axis a little to the N.W. of the line. This brings up the Old Red Sandstone on either side of Fore Hill and near the landing stage. On the other hand, to the north of Wood Hill the strata are folded over so as to dip steeply towards the Severn, the line joining the contorted Bryozoa bed in Wood Hill Bay, and the same bed similarly contorted in the small quarry west of Wood Hill, marking the apex of this steep fold. It may be that the limestone here, as well as being folded, is slightly faulted, down; and there seems to have been a slight shifting down between the beds of Battery Point and those of the woody ridge farther E. along the line where the Dolomitic Conglomerate is shown on the map. These shiftings are, however, of slight amount and of minor importance.

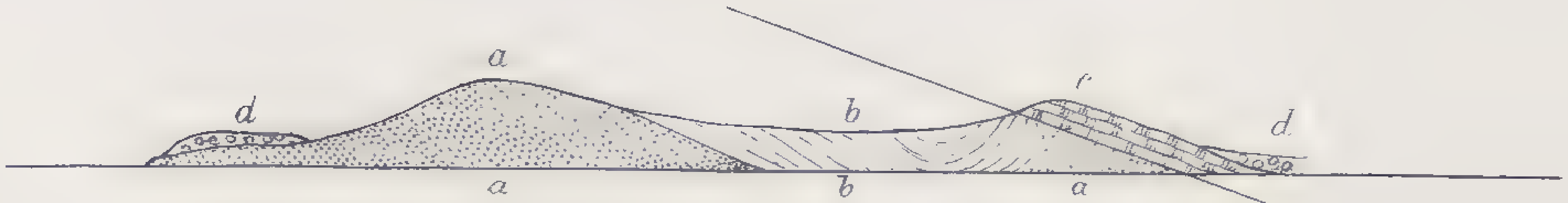
There remains for explanation the Mountain Limestone of Fore Hill. These beds cannot be brought into direct relation with the Old Red Sandstone in the lanes which lie to the north and south of the hill. It is clear that they are brought to their present position by the action of a fault. This was seen by Mr. W. W. Stoddart; and I here reproduce his section from our Proceedings (New Series, vol. i., p. 272), that his interpretation of the facts may be compared with that to which my own investigations have led me (Fig. 2).

It will be seen that, on Mr. Stoddart's view, the reappearance of Old Red Sandstone in the lanes is due to upthrow by the fault, and, although his lines which distinguish the limestone end off against the Old Red, I take it that this limestone is intended to be represented as reposing com-



a Devonian. *b* Carboniferous. C. Trias. *d* Alluvium.

Fig. 2, Stoddart,



a Old Red Sandstone. *b* Lower Limestone Shales. *c*. Mountain Limestone.
d. Dolomitic Conglomerate

Fig. 3, (Lloyd Morgan)

fortably on the Old Red Sandstone. This is in my opinion impossible. The Old Red to the N. dips 75° N.W. The limestone overlying it dips 18° S.E. There is not room for the Lower Limestone Shales between the two. Nor is there any sign of fault between the Old Red Sandstone of the lane, upturned as it is, and the Lower Limestone Shales near the bifurcation of the road. On my view (see Fig. 3) the repetition of the Old Red is due to upfolding of the strata, while the limestone of Fore Hill is brought down by a flat-lying downthrow from the limestone mass which, ere denudation removed it, occupied its normal position over what is now West Hill. The fault is thus analogous to that which brings down the limestone patches in the Clapton-in-Gordano coal-field. And neither in the one case or the other is the throw of very great amount.

The roll of the strata, by which I contend the upbringing of the Old Red Sandstone on either side of Fore Hill to have been effected, is further evidenced by the limestone of the quarry to the S.E. of the map. Did no such roll exist, we should expect upper beds of limestone here. The beds are, however, low down in the Mountain Limestone Series, closely resembling those of the Black Rock quarry in the Avon section. On my last visit to this quarry I found a characteristic fragment of an ichthyodorulite of *Oracanthus* (probably *Milleri*), a form also found in the Black Rock of the Clifton section.

4.—*Conclusion.*

It only remains to point out, in conclusion, that the flexures and faultings described in the last section date back to pre-Mesozoic times—to those periods of disturbance which ushered in and succeeded the deposition of the Permian beds in the northern counties. This is shown by the position of the Dolomitic Conglomerate and Trias. So too with the physical features. The Battery ridge, Wood Hill, and Fore Hill, were already marked out in pre-Mesozoic times. The Portishead-Clevedon ridge was already in existence, at least in a rough-hewn state. Between it and the Clifton-Clevedon ridge were the possibly ice-laden waters of the Old Triassic Lake. But as the ages of Mesozoic times passed by these ridges and islands underwent gradual submergence; and latter-day denudation has been largely occupied in removing the load of secondary rocks beneath which they were thus smothered.

The Bone-Cave or Fissure of Durdham Down.

By E. Wilson, F.G.S., Curator of the Bristol Museum.

THE object of this communication is to rescue from destruction the history of the exploration of the Bone Cave or Fissure on Durdham Down, from which the very interesting series of mammalian remains in the Bristol Museum were obtained.

Notwithstanding the comparatively brief period that has elapsed since these fissures were explored, it is absolutely impossible, owing to the imperfection of the record, to give anything like a complete account of the discoveries that were then made. The fissure itself is no longer visible, and its exact site is now lost. There is also some doubt as to the authenticity of a portion of the animal remains, which are somewhat doubtfully stated to have come from the Durdham Down Cave. It must therefore be understood that the account here given is avowedly imperfect, the materials available for its construction being themselves so imperfect.

In the early part of 1842 * a large and important series of

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* A much earlier discovery of fossil bones in a cavity of Mountain Limestone, "near Clifton, by the turnpike-gate on Durdham Down,"

fossil bones were found in some fissures which were disclosed in working a limestone quarry on Durdham Down.* These were first noticed by Dr. Riley, and subsequently by Mr. Stutchbury. The following is the full report of a communication by Mr. Stutchbury to the Bristol Institution, which appeared in Felix Farley's *Bristol Journal* for December 31st, 1842, with the omission of a portion of the author's introductory remarks and some of the less valuable of his theoretical deductions:—

"At the fourth private meeting of the Philosophical Society, held Thursday, 28th inst. (28th Dec., 1842), Mr. Stutchbury described the nature and contents of the Bone Cavern lately opened on Durdham Down. The first notice of this discovery was made about ten months ago by Dr. Riley, to whom some bones were then brought. No further discoveries appeared until the month of November, when excavations were being made near the position of the cavern. After a consultation amongst some members of

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was made by Mr. Miller about the year 1820. This early find, which consisted of a portion of the hind limb of a horse, held together by a stalactitic cement, is mentioned by Buckland. It cannot, however, be considered a discovery of any particular value, and does not appear to be in any way connected with the later discoveries of Messrs. Riley and Stutchbury. Indeed there is good reason for thinking that the fissures were quite distinct, or, at any rate, that the points of discovery were widely separated from one another.

* Mr. R. Etheridge, F.R.S., who was, I believe, resident in Bristol at the date of this discovery, and who speaks from personal recollection, informs us that "the cave or fissures occurred in one of the large quarries on the Down, between the Black Boy inn, or corner of the Down, and Cook's Folly, in direct line." Is not this the large quarry which once existed in the middle of Durdham Down, near the reservoir, on the south side of the Stoke Bishop road, and which was eventually filled up with the material taken from the excavation for the Cumberland basin?

the Society, it was recommended to prosecute further researches, and the suggestion being adopted by the committee, and a small sum of money voted to defray expenses, it was deemed advisable to place the investigations under the sole direction of Mr. Stutchbury."

After making some further observations on the general subject, Mr. Stutchbury proceeded to the particular description of the discovery on Durdham Down, and exhibited drawings of the quarry and a small model.* He thus describes the fissure: "The cavity is complicated in form, but consists essentially of a perpendicular fissure concurrent with the natural joints of the rock, having a variable length, generally 8 or 10 feet, and traceable to a depth of nearly 90 feet from the surface. The upper part of the fissure has a breadth sufficient to admit animals of moderate size, but at a depth of 12 feet it suddenly contracts to a comparatively narrow cleft, rendered very irregular by perpendicular sharp projections of the rock, and thereby adapted for the passage of living animals downward to a larger cavity, into which the cleft communicates laterally at a depth of 20 feet, and which constitutes the present floor of the quarry; another perpendicular and parallel fissure opens upon the opposite end of the cavity. The bones were found embedded in a mixture of mud and broken pieces of limestone, and occupying the greater part of the first-named fissure, and the whole of the lateral opening. It has been determined that other cavities exist at lower depths, but their contents remain unknown. The following animals have been ascertained to have furnished the bones: Of the carnivora there are eleven or twelve hyænas and one bear, also two wolves and several foxes, both of which bear evidence of later date.

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* *See* p. 36.

The herbivora comprise very numerous examples of bos (wild bull), about five deer, six or seven elephants, varying from extremely young to one very aged, two examples of rhinoceros, and several of hippopotamus. The bones are all attached, and fractured into small bits, and the proportion of teeth and horns to the other parts of the body greatly preponderates.

"In reference to the possible modes of accumulation, the first method by floods is excluded, because, as in all diluvial accumulations, there would have been a mixture of rolled stones of various kinds. If the animals had fallen into the fissures, whole skeletons, or at least all the bones of a single individual, would have been entombed. But so far from this being the case, the receptacle would not contain a number approaching to that of the animals whose remains are there found. But, on the other hand, the theory that the cave was the den of hyænas is consistent with all the observed facts. The habits of these animals to tear up putrid carcasses, to carry off portions to their dens in rocks, to crush with violent force the bones of their prey—for which their jaws are constructed to act with wonderful effect—the gnawed and splintered condition of the bones are circumstances which render the last-named theory highly probable, and worthy to be assumed as the true one. By comparison of the teeth of the fossil hyæna and bear with those of recent animals, their enormous size was strikingly shown; those of the hyæna proved it to have been larger than the largest known species of tiger.

"Mr. Stutchbury then drew attention to the following facts: An elephant's tooth had been broken into two parts, in a direction not corresponding with that of the laminæ of the tooth, but diagonally across the laminæ, and the parts had moved relatively in the plane of the section; another tooth was found crushed and fixed between projecting points of the rock, a cylindrical bone was split longitudinally, and similar movement in the plane of the fracture had occurred. Several other portions of long bones were fractured longitudinally, and the laminæ of a very large elephant's tooth separated; whilst the detached parts in both cases still remained in clear juxtaposition, adhering by thin layers of indurated clay. These appearances suggested the hypothesis of considerable relative movement having taken place in the walls of the fissure and cavern since the deposit of the organic remains, and might have produced the closing of the orifice, and the consequent high preservation of the bones. . . . .

"The meeting closed with a discussion, in which Dr. Riley made some observations on the structure and marks of age exhibited by the teeth of the elephants, and Mr. Austin placed on the table an elaborate plan and sections of the quarry.

"W. S."

I have quoted the foregoing notice so fully, because it is the earliest and only detailed account I can find of the discovery of the Durdham Down Bone Cave. The description given by Mr. Stutchbury must, I think, be acknowledged to be a philosophical one, and his theoretical deductions to be in the main sound. On one or two points we may not now be able entirely to agree with him, and one or two matters remain which still require elucidation.

In the first place it is to be noted that a single fissure only is referred to, although a fissure of a complicated character and duplex form. The osseous remains in the Bristol Museum were mostly labelled "D. D. C. E." or "D. D. C. W.," as if indicating that they were derived from two distinct cavities. There are two models* in the Museum, each of which delineates a fissure. The fissures thus delineated, however, whilst not absolute counterparts, have the same general character, consisting of a narrow, nearly vertical, cleft, opening at the surface of the ground, widening out and divided below by sharp rocky projections, and continuing downwards into a more spacious horizontal chamber, whence a narrow pipe-like fissure proceeds vertically downwards (*see* Plates III. and IV.). † So nearly do the two models agree, both in their general and detailed conformation, that I look upon them as right and left-hand illustrations at two contiguous points of one and the same fissure, which had been cut through and exposed by quarrying operations carried forwards in a transverse direction. One of the models is constructed in two halves, in order to show the continuation of the large lateral cavity, and the parallel perpendicular fissure, also opening out of the surface, as described by Mr. Stutchbury. I infer then that the Durdham Down "Bone Cave" consisted essentially of a *single* line of fissure running roughly east and west; that this fissure was, generally speaking, a narrow cleft, but that it had here and there become widened out laterally, so as to show at least two surface openings of appreciable size; that similar expansions of the fissure occurred at a depth from the surface, which were in

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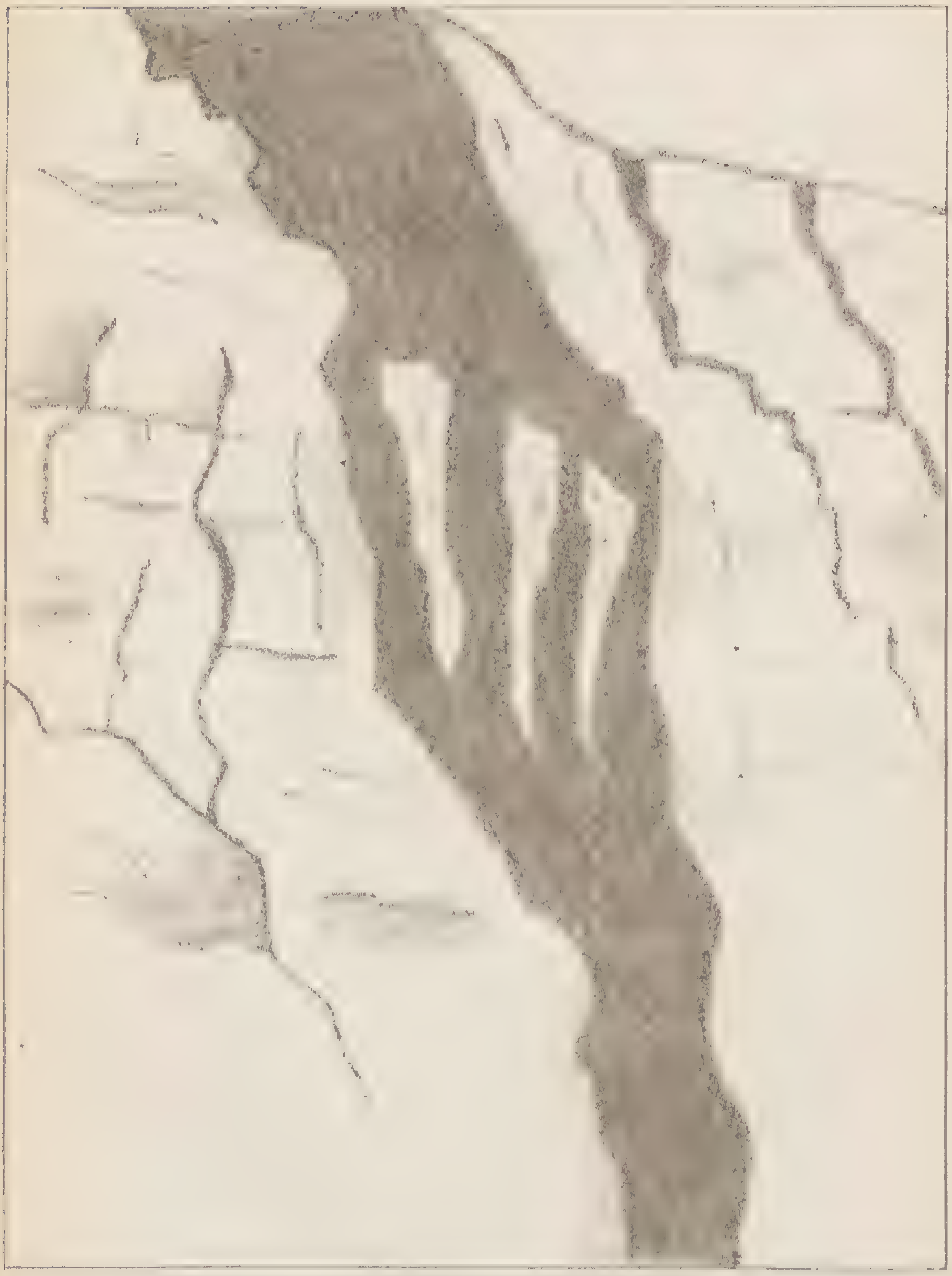
* These models were lately unearthed in the attics of the Institution, and have now been restored and placed in the Museum. I had previously noticed a smaller set of models of the same fissures in a museum cabinet; and I may say that my only means of identifying these models with the Durdham Down fissures is a pencil-note to that effect in an old catalogue of museum duplicates, taken in connection with the description in Mr. Stutchbury's report.

† On one of the models there are figures to show that the depth (from the surface) to the top of the hole (*i.e.*, the pipe-like fissure) was 36 feet, and from this point to the bottom of the hole 37 feet.

Plate II



Durdham Down Cave.
(left hand view.)

Plate IV

Durdham Down Cave.

some cases connected by lateral galleries running along the line of fissure, whilst at certain points distinct pipe-like clefts proceeded vertically downwards to considerable but unproved depths; and that the whole of these cavities, which were in the first place excavated by the chemical erosion of percolating rainwater, had subsequently become more or less completely filled up with cave-earth and stalagmite, in which the bones and teeth of the various animals above referred to were entombed.

Whilst admitting that the balance of evidence is distinctly in favour of the hypothesis that the "Durdham Down Cave" served as a haunt for beasts of prey, we cannot help noticing the somewhat awkward sort of a den such a narrow, vertical, and interrupted cavity would be for an animal like a hyæna. Probably, however, the entrance to the fissure was facilitated by the slope there appears to have been in its walls, and perhaps also by a talus of cave-earth at the period when it was occupied by these animals.

This difficulty appears to have struck an anonymous writer at the time, as will be seen from the following extract from the *The Geologist*, for which I am indebted to the courtesy of Mr. F. W. Rudler, F.G.S., Registrar of the Royal School of Mines:

"*Organic Remains.*—A quantity of bones of the bear, hyæna, hippopotamus, rhinoceros, deer, and elephant, have been discovered in a cave in the Mountain Limestone at Durdham Down, Bristol, and the peculiarity of the circumstance is that they were found in a fissure only, which, as far as can be ascertained, extends a very considerable depth lower than the workmen have ever yet gone. This is not the case in other places, where the bones are all found in caverns, which would appear in some measure to refute the theory of certain philosophers, viz., that these caverns were the resort of the hyæna or tiger (?), where they dragged in and devoured their prey, and afterwards died in the same cave." *

For the same reasons as those given by Mr. Stutchbury, I should be inclined to dissent from the foregoing observation, and to accept the hypothesis that the Durdham Down Cave really did serve as a haunt for hyænas (and at a later date, perhaps, for wolves and foxes), as the correct one to account for the introduction of their osseous contents. As regards the alleged exceptional size of the teeth of the fossil bears and hyænas compared with those of their recent allies, I may observe that so far as the hyæna is concerned there are no teeth in the Bristol Museum of a size sufficient to support this inference, but there is a mandible of a cave bear of average adult size (measuring 1′ 0¾″ from condyles to incisor border, and more than 9″ from condyle to condyle), considerably larger than that of the great polar bear, which might therefore be taken to justify the comparison instituted by Mr. Stutchbury, so far as bears are concerned.

Geologists will not at the present day be prepared to accept the hypothesis of subsequent earth movements in order to account for the fractured elephant's tooth and bones. The fractures are not of that clean-cut kind which a fault would produce; a fall from some height on to a hard, rocky projection would probably suffice to explain the appearances met with.

Although the special object of this communication has now been accomplished, I must not conclude without a brief reference to the lessons, so pregnant with interest, which are to be derived from a consideration of the foregoing facts. For the following generalizations, I may say that I am

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* *The Geologist.* London, vol. ii., 1843, pp. 71, 72.

almost wholly indebted to the writings of Professor W. Boyd Dawkins, F.R.S., to whose very valuable and interesting work, entitled "Cave Hunting," I would refer all who are interested in the subject for fuller information. In the Durdham Down fissure, as in several other British caves, we meet with the remains of such distinctly southern-living forms as the lion, hyæna, and hippopotamus, associated with a number of temperate species, such as the wolf, fox, bison, brown bear, otter, hare, and horse, and at least one distinctly northern animal, the reindeer. This remarkable association in Pleistocene Europe (and Britain) of animals, some of which are now only alive in widely remote parts of the world, and in far-removed latitudes, points to very great geographical and climatal changes since that period. Nearly all the temperate, northern and mountainous species of Pleistocene Europe can be traced to northern and central Asia, whilst the headquarters of the southern animals are in Africa and South Asia. Europe was then intimately connected with Africa on the south, and with Asia on the east, and offered no barriers to the migration of Asiatic and African animals, as far to the west as Britain and Ireland. The apparent anomaly of an intermixture of southern with temperate and northern forms in the same district, and even in the same cave deposit, is in all probability to be explained by extreme seasonal, coupled with considerable secular, variations in the climate of the same regions, leading to the constant swinging backwards and forwards of the northern and southern forms over the middle or normally temperate zone. Throughout this middle zone, comprising France, Germany, and the greater part of Britain, the climate was colder in winter than now, and warmer in summer, as it is at the present day in Central Asia and North America, where large tracts of land extend from the polar region towards the equator. In the summer time the southern species would pass northwards, and in the winter time the northern animals would swing southwards, and thus occupy at different times of the year the same tract of ground as is now the case with the elks and reindeer. It must not, however, be supposed that the southern animals migrated from the Mediterranean as far north as Yorkshire, or the northern as far south as the Mediterranean, in one and the same year. There were secular changes of climate in Pleistocene Europe, and while the cold was at its maximum the Arctic animals arrived at the southern limit, and while it was at its minimum the spotted hyæna, hippopotamus, and other southern animals roamed to the north limit. Thus every part of the middle zone has been successively the frontier between the northern and southern groups of animals, and consequently their remains are mingled together in the caverns and river deposits under conditions which prove them to have been practically contemporaneous in the same region. In this way the association of northern and southern animals may be explained, namely, by their migrations according to the seasons, and their association over so wide an area as the middle zone, by the secular changes of climate, by which each part of that zone in turn was traversed by the advancing and retreating animals.

The identity of the British Pleistocene fauna with that of the Continent leads irresistibly to the conclusion that in the Pleistocene age Britain was connected with the adjacent countries by " a bridge of land," over which the wild animals had free means of migration. Soundings show that Britain and Ireland constitute merely the uplands of a plateau now submerged to the extent of about 100 fathoms on the margin of the Atlantic. An elevation to this extent would convert Ireland, England, and the surrounding seas into a part of the Continent; whilst an elevation of only 20 or 30 fathoms would connect Britain to Europe, and 40 or 50 Ireland also. The discovery of the mammoth, rhinoceros, horse, Irish elk, wolf, lion, and bear on so small an island as Caldy (near Tenby), is cited by Boyd Dawkins, to show that a considerable change has taken place in the relation of the land to the sea in that district since those animals were alive. It would have been impossible for so many and so large animals to have obtained food on so small an island. It may therefore reasonably be concluded that when they perished in the fissures, Caldy was not an island, but a precipitous hill, overlooking the broad valley now covered by the waters of the Bristol Channel, but then affording abundant pasture to large numbers of herbivorous mammalia.

The same inference may also be drawn from the vast numbers of animals found in the Gower caves (near Swansea) which could not have been supported by the scant herbage of the limestone hills of that district.* We must therefore picture to ourselves a fertile plain, occupying the whole of the Bristol Channel, and supporting herds of reindeer, horses, and bisons, many elephants and rhinoceroses, and now and then traversed by a stray hippopotamus, which would afford abundant prey to the lions, bears, and hyænas inhabiting all the accessible caves in the neighbourhood, as well as to their great enemy and destroyer, man.

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* At the period of which we are speaking, Durdham Down (or, as the level plateau going by that name might more appropriately be designated "Durdham Plains") would form, not as it now does, a comparatively low-lying plain about 300 feet high, but an elevated tableland nearly one thousand feet above the level of the sea.

APPENDIX I.

SUMMARIZED LIST OF MAMMALIAN REMAINS FROM THE DURDHAM DOWN BONE CAVE IN THE BRISTOL MUSEUM.

Elephas antiquus, Falc. ("straight-tusked elephant"). 2nd and 3rd milk molars, and 5 (?) molars of adult animals, one of which is transversely and obliquely fractured, and others broken or crushed; most, if not all...	E.
Elephas primigenius, Blum. ("the mammoth"). Molars (authorities, W. Boyd Dawkins, 1874; E. T. Higgins, 1847). It is doubtful whether any of the elephant's teeth from the Durdham Down Cave, in the Bristol Museum, belong to the mammoth	D.D.C.
Elephas, sp.? 5 molars, including E. antiquus and E. primigenius (?) Probably from	D.D.C.
1 penultimate phalanx—probably from ...	D.D.C.
Hippopotamus amphibius, var. *major*, Cuv. ("African hippopotamus"). 13 molars and premolars, 4 fragments of canine and 1 incisor; metatarsal bone	E.
Rhinoceros hemitœchus, Falc. 6 molar teeth, of which 4 are in pairs; calcaneum	E.
Rhinoceros tichorhinus, Cuv. = *R. leptorhinus*, Owen ("woolly rhinoceros"). 1st milk molar, right side...	E.
Bos, sp.? Molar and premolar teeth, radius, metacarpals, longitudinally fractured, metatarsal ...	E.
Cervus, sp.? Antlers of deer; lower jaw	W.
Cervus tarandus, Linn. ("reindeer") (authority, W. Boyd Dawkins, 1874)	D.D.C.

Equus caballus, Linn. (= *E. fossilis*, Cuv.) ("horse") (authorities, Mus. Cat., 1846; E. T. Higgins, 1847; W. Boyd Dawkins, 1874); not in Museum 1886 D.D.C.

Sus, sp. ("boar"). Molars and canines (authority, Mus. Cat., 1846); not in Museum, 1886 ... D.D.C.

Lepus timidus, Linn. ("hare"). Incisors ... ... D.D.C.

Arvicola, ? sp. ("vole" ?). Incisors ... ... D.D.C.

Mus musculus, Linn. ("mouse") (authority, Mus. Cat., 1846); not in Museum 1886 ... ... D.D.C.

Felis leo, var. *spelœa*, Linn. ("lion"). Canine tooth (authority, W. Boyd Dawkins and W. A. Sandford, 1866 and 1874). In collection of Earl of Enniskillen, Florence Court, Ireland; not in Museum 1886 ... ... D.D.C.

Ursus Arctos, Linn. ("brown bear"). Skull with 3 pair of molars, canines, and incisors; portion of lower jaw; metacarpals and phalanges ... W.

Ursus spelœus (?) Goldf. ("cave bear"). Mandible with 3 pairs of molars and pair canines; numerous canine, premolar, and molar teeth of both jaws; portions of upper and lower jaws, with molars; cervical and dorsal vertebræ; astragalus, carpals, metacarpals, and phalanges ... ... W.

Hyæna crocuta, Linn., var. *spelœa*, Goldf. ("spotted hyæna"). Portions of jaws, with all classes of teeth in position, and isolated premolars and canines of both jaws ... ... E.

Vulpes vulgaris, Owen ("fox"). 9 rami of lower jaws, with all classes of teeth in position; 5 portions of upper jaws, with teeth; cervical vertebræ, ilia, sacrum, femur, tibia; ribs, humerus, radius, ulna, os calcis, metacarpals and phalanges, astragalus ... ... W.

Canis lupus, Linn. ("wolf"). Upper and lower jaws, with molar and premolar teeth in position; cervical, dorsal, and lumbar vertebræ; scapula, humerus, radius, ulna, metacarpals; femur, tibia, fibula, metatarsals and phalanges ...	W.
Meles taxus, Linn. ("badger"). Right and left mandible (?)	D.D.C.
Lutra vulgaris, Owen ("otter"). Fragments of upper jaw and humerus	W.
Mustela, sp. Canine	D.D.C.
Three long bones exhibiting marks of gnawing by a rodent	W.
Two bones exhibiting marks of gnawing by hyæna	E.
A few undetermined bones, more or less fragmentary.	

NOTE.—The letters "W." and "E." indicate that the remains referred to were respectively derived from the eastern and western cavities, whilst the letters "D.D.C." indicate that the remains thus lettered came from the "Durdham Down Cave," but that the precise spot from which they were derived was not recorded.

APPENDIX II.

LIST OF THE PRINCIPAL PUBLICATIONS REFERRING TO THE ORGANIC REMAINS DERIVED FROM THE BONE CAVE ON DURDHAM DOWN.

1842.—"*W.S.*," *in Felix Farley's Bristol Journal for* 31*st December*, 1842.

1843.—Anonymous Organic Remains (Durdham Down), "Geologist," vol. ii. p. 71.

1846.—Owen, Richard, F.R.S., "British Fossil Mammals and Birds," pp. 102, 155, 345, 410.

1864.—Dawkins, W. Boyd, F.R.S., British Association Report, Bath Meeting, 1864.

1865.—Austin, T., F.G.S., Fort Major, "The Millstone Grit and its Fossils," p. 29.

1866 et seq.—Dawkins, W. Boyd and Sandford, W. A. Palæontographical Society, "Pleistocene Mammalia," p. 153, etc.

1868.—Falconer, Hugh, "Palæontological Memoirs by Charles Murchison, M.D.,F.R.S.," vol. ii. pp. 178, 179, 323, 327, 349, and 534.

1874.—Dawkins, W. Boyd, F.R.S., "Cave Hunting," pp. 291, 292.

1876.—Horace B. Woodward, F.G.S., "Geological Survey: Memoir on East Somerset and the Bristol Coalfields," pp. 188, 189.

1877–1881.—Adams, A. Leith, M.B., F.R.S., Palæontographical Society, "British Fossil Elephants," pp. 7, 72.

The Fungi of the Bristol District.

PART IX.

By CEDRIC BUCKNALL, Mus. Bac.,

Corresponding Member of the Cryptogamic Society of Scotland.

1198. PSEUDOVALSA FUSCA, *Bucknall.* Plate V., fig. 1.*

Pustules small, numerous, erumpent. Perithecia globose, fibrillose, immersed in a brownish orbicular stroma; necks elongated, slender, cylindrical, converging; ostiola scattered over the brownish, depressed disc. Asci clavate, shortly stipitate, 65–100 × 10; paraphyses abundant, filiform. Sporidia oblong, 3-septate, brown, 17 × 5.

On twigs of *Acer pseudoplatanus.*

1216. SORDARIA SPARGANICOLA, *var.* VELATA, *Bucknall.* Plate V., fig. 2.

Perithecia subcuticular, depressed; ostiola conical, erumpent. Asci 200 × 17. Sporidia, 18–20 × 9–10.

On a dead umbelliferous stem.

Differs from the type in the depressed perithecia, and smaller asci and sporidia, but can scarcely be separated as a distinct species.

1218. SPORORMIA SECEDENS, *Bucknall.* Plate V., fig. 3.

Perithecia superficial, ovate, carbonaceous, fragile, ⅓–½ mm: ostiola inconspicuous, not prominent. Asci broadly clavate, stipitate, apex

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* The figures in Plates V. to VIII. are all drawn to scale, the asci and sporidia being magnified about 830, and the perithecia 70 diameters.

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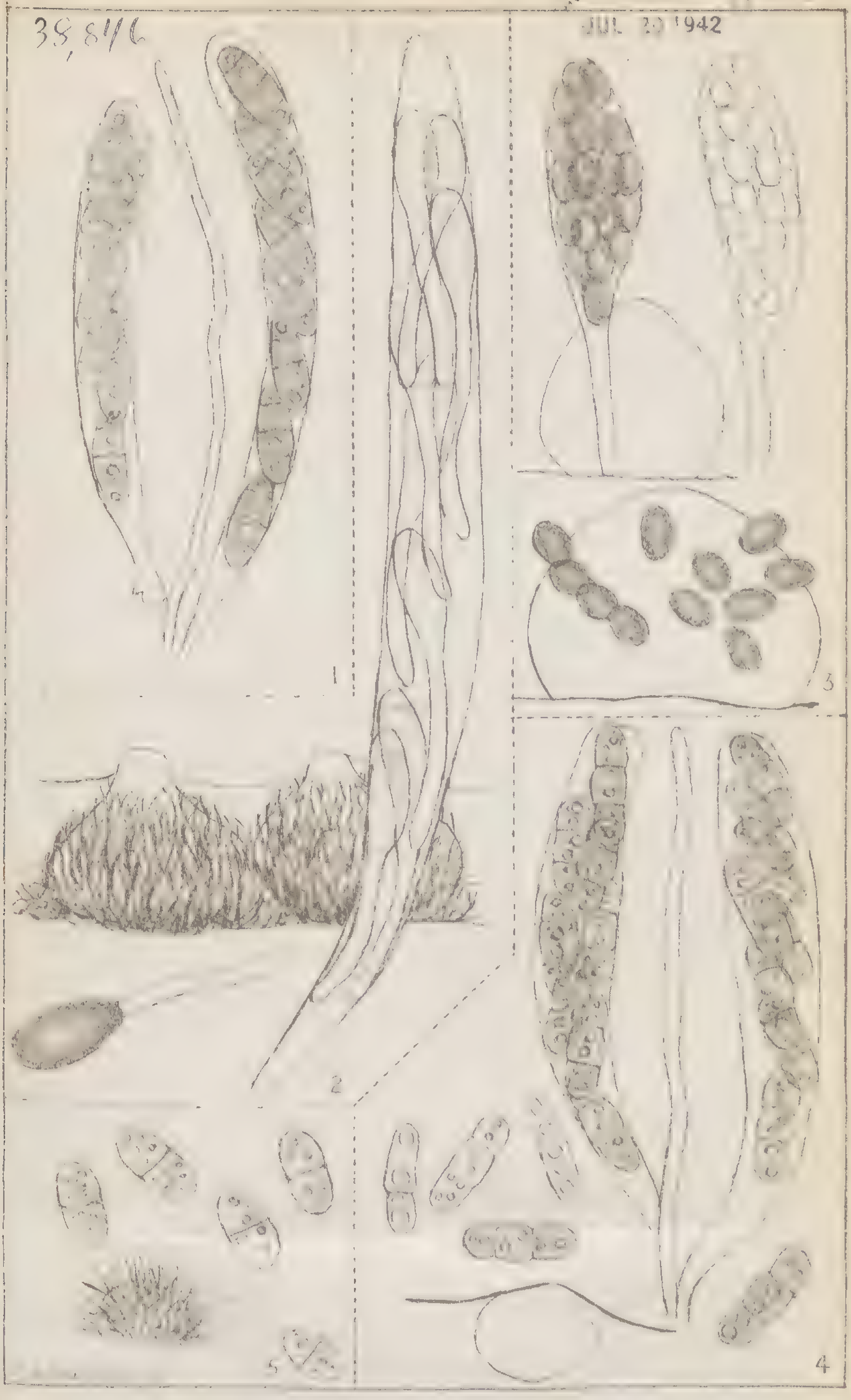

1 *Pseudovalsa fusca* [illegible] 3 [illegible]
2 *Sordaria spargania* [illegible] 4 [illegible]
5. *Chaetodiplodia caulina*, [illegible]

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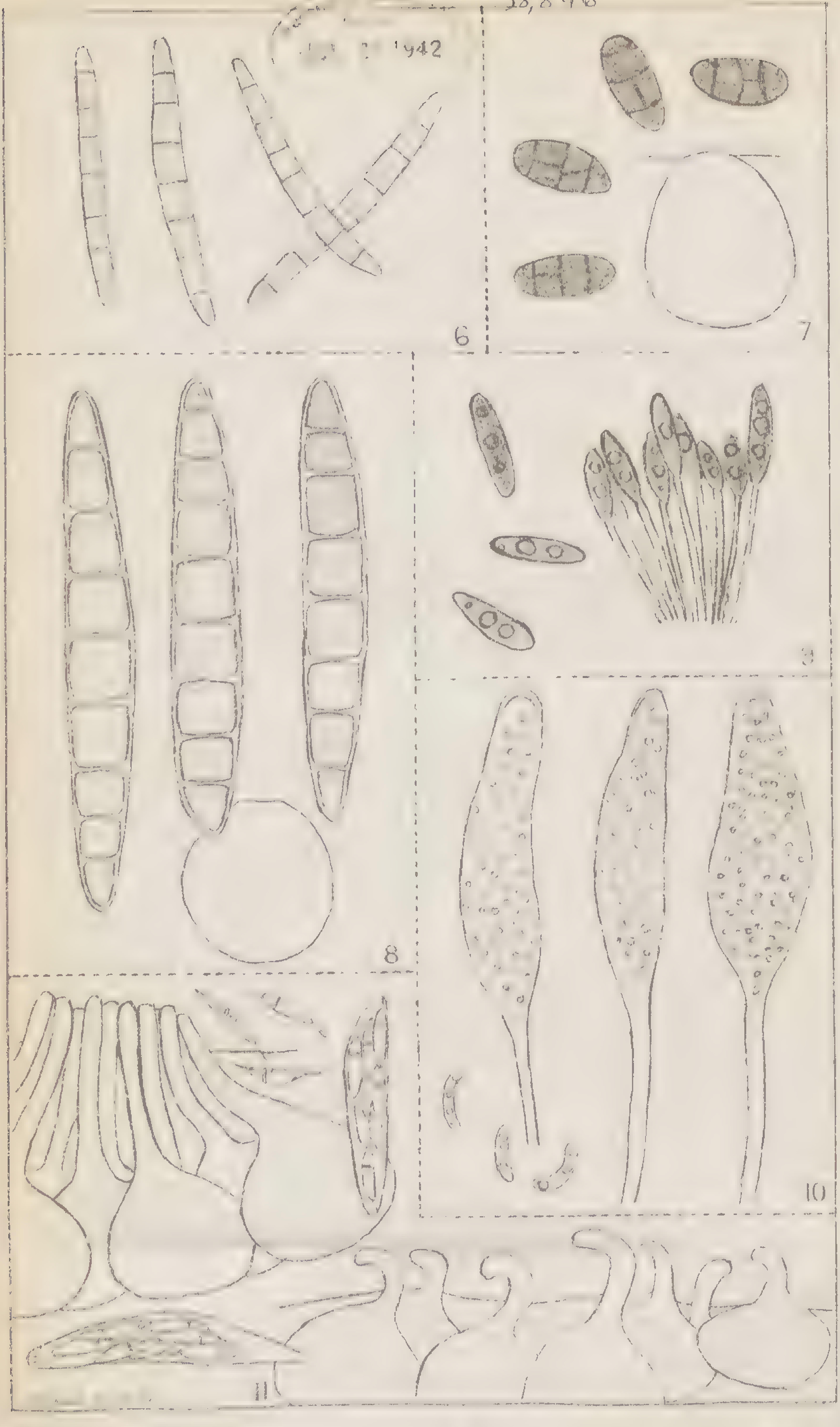

6 *Hendersonia [illegible]* [illegible]
7. *Hendersonia Phragmitis* [illegible]
8. *Staganospora gigaspora* [illegible]
9 *Melanconium [illegible]* [illegible]
10 *Valsa [illegible]* [illegible]
11 *Valsa [illegible]* [illegible]

rounded, 75×17. Sporidia tetramerous, olivaceous-black, breaking up into elliptical segments while still in the asci, 28μ long, segments 7–9×5.

On a decorticated stick.

The sporidia break up into segments so soon that their compound structure can only be discovered by examination in a very young stage. The old and broken perithecia are seen to be tinged with pink inside.

The following, as far as I can discover, have not previously been recorded as British:—

1155. Cortinarius (Inoloma) penicillatus, *Fr. Hym. Eur., p. 365.*

1164. Chætodiplodia caulina, *Karst. Sacc. Syl. III., 2091.* Plate V., fig. 5.

On dead stems of *Chenopodium.*

1168. Hendersonia riparia, *Sacc. Syl. III., 2386.* Plate VI., fig. 6.

On *Carex riparia.*

1169. Hendersonia Phragmitis, *Desm. Sacc. Syl. III., 2393.* Plate VI., fig. 7.

1171. Stagonospora aquatica, *var.* junciseda, *Sacc. Syl. III. 2470.*

1172. Stagonospora gigaspora, *Niessl. Sacc. Syl. III., 2473.* Plate VI., fig. 8.

On *Carex riparia.*

1175. Septoria Medicaginis, *Rob. & Desm. Sacc. Syl. III., 2747.*

1178. Melanconium Typhæ, *Peck. Sacc. Syl. III., 3987.* Plate VI., fig. 9.

1193. Valsa (Coronophora) gregaria, *Lib. Sacc. Syl. I., 413.* Plate VI., fig. 10.

*1197. Valsa (Chorostate) nidulans, *Niessl, Sacc. Syl. I., 2428.* Plate VI., fig. 11.

1201. Diaporthe (Tetrastaga) revellens, *Nke. Sacc. Syl. I., 2575.*

1202. Diaporthe (Tetrastaga) Beckhausii, *Nke. Sacc. Syl. I., 2592.*

1203. Diaporthe (Tetrastaga) circumscripta, *Otth. Sacc. Syl. I., 2593.*

1208. Lophiostoma (Lophiosphæra) pulveracea, *Sacc. Syl. II., 5414.* Plate VII., fig. 12.

1209. Lophiostoma (Lophiotrema) vagabundum, *Sacc. Syl. II., 5435.* Plate VII., fig. 13.

1222. Leptosphæria nectrioides, *Speg. Sacc. Syl. II., 3016.* Plate VII., fig. 14.

1223. Leptosphæria Microscopia, *Karst. Sacc. Syl. II., 3069.* Plate VII., fig. 15.

1224. Leptosphæria vagabunda, *Sacc. Syl. II., 2963.* Plate VII., fig. 16.

1227. Ceriospora xantha, *Sacc. Syl. II., 3520.* Plate VIII., fig. 17.

1230. Pleospora vagans, *Niessl. Sacc. Syl. II., 3804.* Plate VIII., fig. 18.

1235. Sphærella Tassiana, *De Not. Sacc. Syl. 1., 2058.* Plate VIII., fig. 19.

On *Typha latifolia.*

1145.	Agaricus (Lepiota) excoriatus, *Schaeff.*	Sea Mills,	Oct.,	1885.
	*Agaricus (Tricholoma) argyraceus, *var.* virescens, *Cooke, Illus., pl. 641.*	Leigh Woods.		

This well-marked variety occurs abundantly every year in the Leigh Woods. It was recorded at No. *696 as *Agaricus terreus, var. argyraceus.*

1146.	Agaricus (Clitocybe) nebularis, *Fr.*	Blaise Castle Woods,	Oct.,	1885.
1147.	Agaricus (Clitocybe) opacus, *With.*	,,	,,	,,
1148.	Agaricus (Clitocybe) incilis, *Fr.*	Durdham Down,	Sept.,	,,

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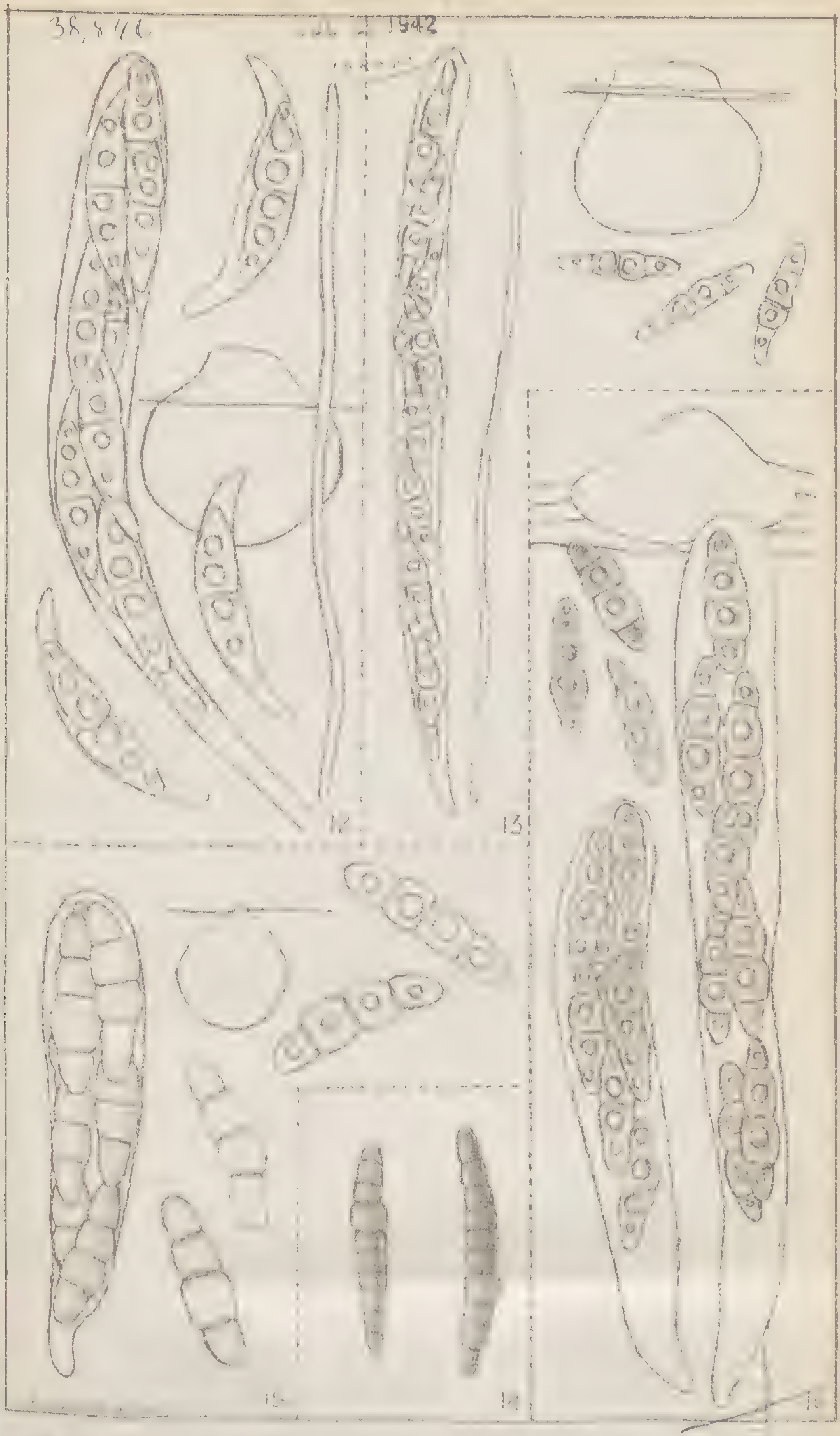

12 [illegible] 14 [illegible]
13 [illegible] 16 [illegible]
15 [illegible]

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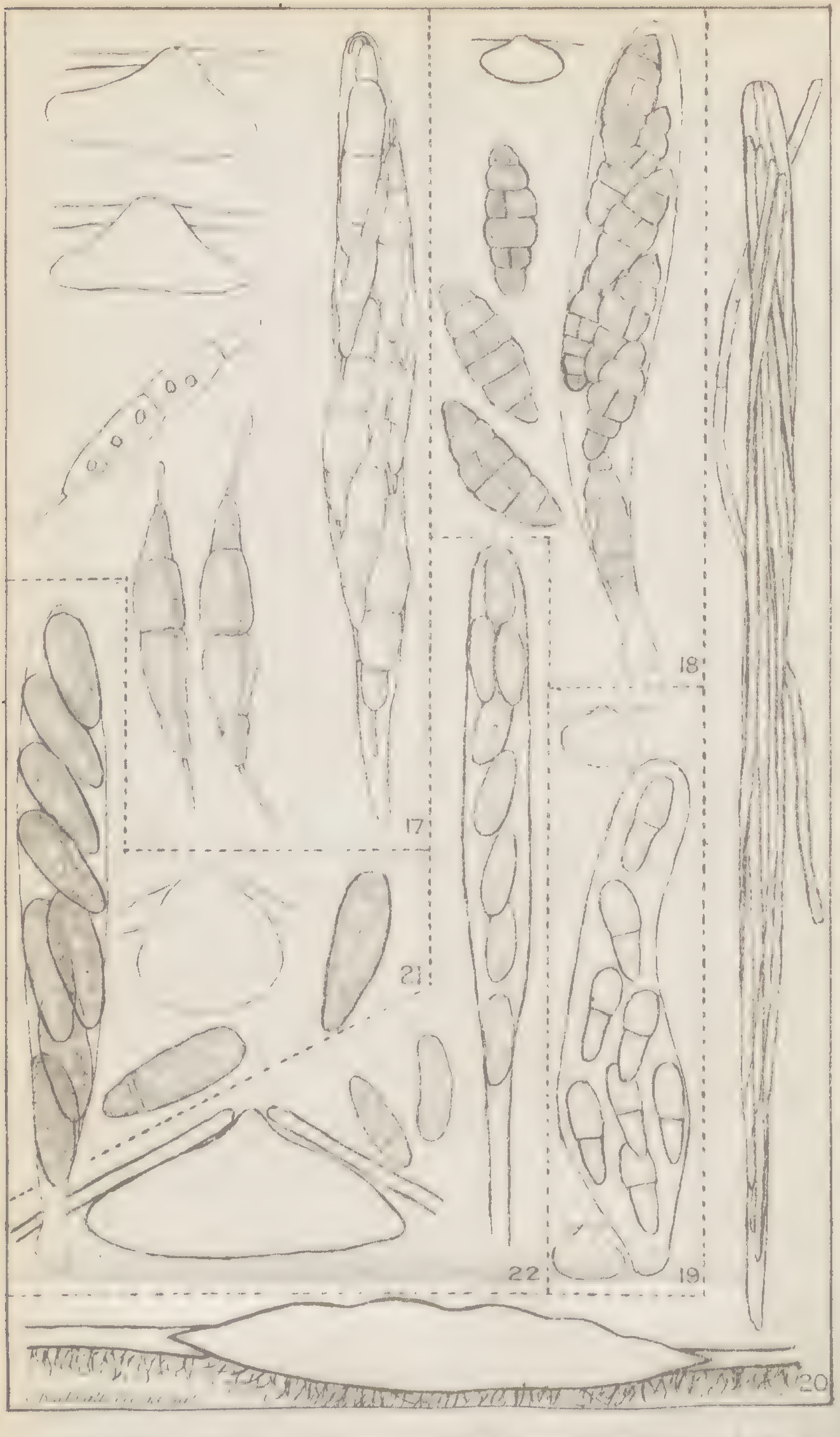

17. *Ceriospora xantha*, [illegible]
18. *Pleospora vagans*, [illegible]
19. *Sphaerella Tassiana*, D. [illegible]
20 *Hypospila viburni* [illegible]
21. *Didymella saepincoliformis*. De
22. *Metasphaeria corticola*. Sacc. & Speg.

	*Agaricus (Clitocybe) geotrupus, *Bull.*	Westridge Wood,	Oct.	1882.

The locality given at vol. ii. pt. iii. p. 342 is incorrect.

1149.	Agaricus (Nolanea) piscіodorus. *Ces.*	Kingsweston,	Sept.,	1885.
1150.	Agaricus (Galera) sparteus *Fr.*	Durdham Downs,	Aug.	1884.
1151.	Agaricus (Psalliota) sylvaticus, *Schaeff.*	Kingsweston,	Oct.,	1885.
1152.	Agaricus (Hypholoma) egenulus, *B. & Br.*	Durdham Downs,	Sept.,	1883.
1153.	Agaricus (Psathyra) spadiceogriseus, *Schaeff.*	Berwick Wood,	Feb.,	1884.
1154.	Agaricus (Psathyra) semivestitus, *B. & Br.*	Stapleton,	Oct.,	1885.
1155.	Cortinarius (Inoloma) penicillatus, *Fr.*	Blaise Castle Woods,	Sept.,	1884.
1156.	Cortinarius (Dermocybe) ochroleucus, *Fr.*	Leigh Woods,	Oct.,	1885.
1157.	Continarius (Hydrocybe) saturninus, *Fr.*	” ”	”	”
1158.	Russula consobrina, *Fr.*	Frenchay,	,,	1884,
1159.	Cantharellus aurantiacus, *Fr.*	Abbott's Leigh,	Sept.,	1885.
1160.	Polyporus intybaceus, *Fr.*	Frenchay,	,,	1884.
1161.	,, cuticularis, *Fr.*	Stapleton,	Autumn,	,,
1162.	Cyathus vernicosus, *D. C.*	Clifton (Mr. E. Wheeler)	May,	1885.
1163.	Diplodia Scheidweileri, *West. Sacc. Syl.* III. 1830; *Cooke, Sphærops Grev.* XIV., p. 61.	Leigh Woods,	April,	,,
1164.	Chætodiplodia caulina, *Karst.*	The Avon, G.	Spring,	1886.
1165.	Hendersonia sarmentorum, *West.*	Blaise Castle,	Dec.,	1885.

On dead twigs of *Berberis vulgaris.*

1166.	Hendersonia Rubi, *West.*	Blaise Castle,	May,	1885.

1167.	Hendersonia salicina, *Vize.*	Glen Frome,	Mar., 1885.
1168.	„ riparia, *Sacc.*	The Avon,	Spring, 1880.
1169.	„ Phragmitis, *Desm.*	„	Summer, 1885.
1170.	Stagonospora Typhoidearum, *Desm.*	Ham Green,	Sept. „
1171.	Stagonospora aquatica, *var.* junciseda, *Sacc.*	Westbury,	April, 1878.
1172.	Stagonospora gigaspora, *Niessl.*	The Avon, S.	Spring, 1880.
1173.	Ceuthospora phacidioides, *Grev.*	Stapleton,	Jan. 1877.
1174.	Darluca filum, *Cast.*	Berwick Wood,	Mar. 1878.
1175.	Septoria Medicaginis, *Rob. & Desm.*	Leigh Woods,	July, „
1176.	Septoria primulæ, *Buckn.*, *Grev.* XIV., p. 40.	Berwick Wood,	Mar., 1885.
1177.	Septoria atriplicis, *West.*	The Avon,	July, 1883.
1178.	Melanconium Typhæ, *Peck.*	Mangotsfield,	April, 1886.
1179.	Torula abbreviata, *var.* sphæriformis, *B. & Br.*	The Avon, S.	Dec. 1884.
1180.	Tetraploa aristata *B. & Br.*	Ham Green,	Jan., 1886.
1181.	Puccinia tripolii, *Wallr.*	Sea Mills,	Sept., 1885.
1182.	Fusarium equiseti, *Desm.*	Leigh Woods,	June, 1882.
1183.	Peziza venosa, *var.* ancilis, *Rehm.*	Blaise Castle,	May, 1885.
1184.	Peziza furfuracea, *Fr.*	Ham Green,	Sept., „
1185.	Patellaria connivens, *Fr.*	Leigh Woods,	Feb., 1886.

On dead stems of *Rubus Idæus.* Asci 125 × 17; sporidia, 32 × 7.

1186.	Hysterium repandum, *Blox.*	Leigh Woods,	— 1883.
1187.	Trochila laurocerasi, *Fr.*	„	— 1885.
	*Hypospila viburni, *Bucknall, No. 1080 ante,* Plate VIII., fig. 20.		
1188.	Hypoxylon cohærens, *Fr.*	Blaise Castle,	Dec. „

1189.	Diatrype brassicæ, *Cke., Grev. XIII., p. 100.*	Clifton,	June,	1885.
1190.	Diatrype berberidis, *Cke. Grev. XIV., p. 14.*	Blaise Castle,	May,	1884.
1191.	Eutypa lata, *Pers.*	Stapleton,	Spring,	1880.
1192.	,, leioplaca, *Fr.*	Leigh Woods,	May,	,,
1193.	Valsa (Coronophora) gregaria, *Lib.*	Brentry,	June,	1885.
1194.	Valsa (Leucostoma) nivea, *Hoffm.*	Clifton,	July,	1879.
1195.	Valsa (Cryptosporella) hypodermia, *Fr.*	Blaise Castle,	May,	1885.
1196.	Valsa (Cryptospora) suffusa, *Fr.*	Ham Green,	Sept.,	,,
1197.	Valsa (Chorostate) hippocastani, *Cooke. Grev., XIII., p. 98.*	Stoke,	April,	1886.
	* Valsa (Chorostate) nidulans, *Niessl.*	Leigh Down,	,,	1883.

On dead stems of *Rubus Idæus.* Referred to *Sphæria rostellata Fr.* at No. 1083.

1198.	Pseudovalsa fusca, *Bucknall.*	Leigh Woods,	—	1881.
1199.	Fenestella princeps, *Tul.* *Valsa fenestrata, B. & Br.*	Durdham Downs,	Sept.,	1885.
1200.	Diaporthe (Tetrastaga) inæqualis, *Curr.*	Hanham,	Nov.,	,,
1201.	Diaporthe (Tetrastaga) revellens, *Nke.*	Leigh Woods,	June,	,,
1202.	Diaporthe (Tetrastaga) Beckhausii, *Nke.*	Ashton,	July,	,,
1203.	Diaporthe (Tetrastaga) circumscripta, *Otth.*	Blaise Castle Woods,	April,	1883.
1204.	Cucurbitaria cupularis, *Fr.*	Near Coombe Dingle,	Dec.,	1885.
1205.	Massaria fœdans, *Fr.*	Blaise Castle,	May,	,,

1206. Massaria inquinans, *Tode.* Leigh Woods, Mar. 1880.

On *Viburnum lantana.*

1207. Massaria Currei, *Tul.* Blaise Castle, May, 1885.

1208. Lophiostoma (Lophiosphæra) pulveracea, *Sacc.* Pill, Jan., 1886.

*Lophiostoma (Lophiotrema) nucula, *Fr.* Sandy Lane, May, 1878.

1209. Lophiostoma (Lophiotrema) vagabundum, *Sacc.* The Avon, Summer, 1885.

On dead stems of *Spiræa ulmaria.*

1210. Lophiostoma fibritectum, *Berk.* Black Rock Quarry, July, 1885.

1211. Lophiostoma caulium, *Fr.* Black Rock Quarry, May, „

1212. „ arundinis, *De Not.* Ashton, Dec., „

1213. Lophiostoma (Lophidium), compressum, *Pers.* Cheddar, July, 1881

1214. Psilosphæria pulviscula, *Curr.* „ „

1215. Psilosphæria obducens, *Fr.* Leigh Road, Spring, 1880.

1216. Sordaria sparganicola, *var.* velata, *Bucknall.* The Avon, S., June, 1885.

1217. Sordaria lanuginosa, *Pr.* „ G., Sept., „

1218. Sporormia secedens, *Bucknall.* Black Rock Quarry. May, „

1219. Melanomma fuscidulum *Sacc.* Leigh Woods, Feb. 1878.

*Ohleria obducens, *Winter.* „ „ „ 1877.

Named incorrectly as *Sphæria pomiformis* at vol. II. pt. ii. p. 349.

*Anthostoma italicum *Sacc.* The Avon, S. June, 1885.

On dead stems of *Eupatorium cannabinum.*

1220.	Didymosphæria conoidea, _Niessl., Grev., XIV., p. 41._	Leigh Woods,	June, 1882.
1221.	Zignoella seriata, _Curr._	Stapleton,	July, 1879.
	*Didymella sæpincoliformis, _De Not. No. 1142 ante._, Plate VIII., fig. 21.		
	*Leptosphæria Michotii, _West._ Plate V., fig. 4.	Blaise Castle,	May, 1885.

On twigs of _Berberis vulgaris._

The occurrence of this species on a dicotyledonous shrub is interesting, it having been previously recorded only on herbaceous monocotyledons, Saccardo giving Juncus, Andropogon, Scirpus, etc., as its hosts.

It is also instructive, as showing that too much reliance must not be placed on the habitat of fungi in the determination of species, when the same plant may be found on members of such distantly related natural orders as _Berberideæ_ and _Gramineæ._ Another species which Saccardo places amongst those growing on monocotyledons is _Pleospora rubicunda, Niessl._, and this I have lately met with on a _Chenopodium._

1222.	Leptosphæria nectrioides, _Speg._	Leigh Wood,	May, 1885.

On _Clematis vitalba._

1223.	Leptosphæria microscopica, _Karst._	Black Rock Quarry,	Jan., 1886.
1224.	Leptosphæria vagabunda, _Sacc._	Leigh Woods,	Feb., ,,
1225.	Leptosphæria arundinacea _var._ Godini, _Desm._	St. Philip's Marsh,	July, 1882.
	*Metasphæria corticola, _Sacc. & Speg. No. 1143 ante_, Plate VIII., fig. 22.		
1226.	Sphærulina intermixta, _B. & Br., var. Corni._		
1227.	Ceriospora xantha, _Sacc._	The Avon,	Jan., 1886.
1228.	Pleospora vulgaris, _Niessl._	Blaise Castle,	May, 1885.
1229.	,, rubicunda, _Niessl._	Aust, The Avon,	June, 1881. Feb. 1886.

On _Phragmites_ and _Chenopodium._

1230.	Pleospora vagans, *Niessl.*	Black Rock Quarry,	Spring, 1886.
1231.	Ophiobolus urticæ, *Rabh.*	Blaise Castle,	May, 1885.
1232.	„ vulgaris, *Sacc.*	The Avon,	June, „
1233.	Sphærella punctiformis, *Pers.*	Brockley Combe,	May, 1879.
1234.	Sphærella fagi, *Auersw.*	Brockley Combe,	„ „
1235.	Sphærella Tassiana, *De Not.*	Ham Green,	Feb., 1886.
1236.	Sphærella juncina, *Auersw.*	Westbury,	July, 1878.
1237.	Venturia ditricha, *Fr.*	Durdham Downs,	Mar., 1879.
*	„ inæqualis, *Cke.*	Leigh Woods,	Mar., 1886.
1238.	Pyrenophora phæocomoides, *Sacc.*	Black Rock Quarry,	July, 1885.

On *Tanacetum vulgare.*

1239.	Pyrenophora calvescens, *Fr.*	The Avon,	Feb., 1886.
1240.	Anixia perichœnoides, *Cooke.*	Cotham,	Mar., 1880.

With *Peziza misturæ* on a composition spread on apple trees (A. Leipner, Esq.).

Rainfall at Clifton in 1885.

By GEORGE F. BURDER, M.D., F.R. Met. Soc.

TABLE OF RAINFALL.

	1885.	Average of 33 years.	Departure from Average.	Greatest fall in 24 Hours.		Number of days on which ·01 in. or more fell.
				Depth.	Date.	
	Inches.	Inches.	Inches.	Inches.		
January . .	2·735	3·303	−0·568	0·533	30th	14
February . .	3·345	2·386	+0·959	0·517	26th	17
March . . .	1·221	2·162	−0·941	0·360	3rd	10
April . . .	2·181	2·075	+0·106	0·480	1st	13
May . . .	3·256	2·369	+0·887	0·541	21st	25
June . . .	1·733	2·649	−0·916	0·375	23rd	11
July . . .	0·974	2·942	−1·968	0·563	18th	6
August . .	2·323	3·494	−1·171	0·649	26th	11
September .	4·840	3·393	+1·447	2·048	10th	22
October . .	4·525	3·724	+0·801	1·019	9th	22
November .	6·425	2·992	+3·433	2·050	29th	14
December .	1·235	2·826	−1·591	0·409	5th	15
Year . . .	34·793	34·315	+0·478	2·050	Nov. 29th	180

Remarks.—The rainfall of 1885 presents no very remarkable features. Since the unprecedented downpour of 1882, when over 48 inches were registered at Clifton, the yearly totals have shown singularly little deviation from the average. The average annual fall at Clifton, as deduced from observations extending without interruption over thirty-three years, is 34·315 inches. The fall in 1883 was 34·792 inches, in 1884, 33·392 inches, and in 1885, 34·793 inches. It may also be noted that in each of these three years six months have had an excess of rain and six a deficiency.

In 1885, during the early part of the year, the dry and wet months occurred in regular alternation. June, July, and August were all relatively dry; September, October, and November were all wet. The driest month was July, with less than an inch of rain, falling on six days only; the wettest was November, with nearly six and a half inches. This was more than double the average for November, and was, in fact, the largest amount that has been recorded here in that month.

Two very heavy diurnal falls were registered during the year: on the 10th of September, 2·048 inches; and on the 29th of November, 2·050 inches.

No measurable depth of snow lay on the ground either in the former or in the latter part of the year.

Meteorological Observations,

AS REGARDS

Temperature, taken at Clifton, 1885.

By H. B. JUPP, M.A., F.R. Met. Soc.

THE observations from which the following tables are derived have been taken at 8.30 a.m. daily. The Thermometers used, with the exception of that for the Ground Temperature, are kept in a Stevenson cage; and in all the readings the requirements of the Royal Meteorological Society are strictly complied with.

1885 TEMPERATURES.

MONTH.	Maximum in Shade.		Minimum in Shade.		Mean in Shade.	Minimum, on Ground Lowest recorded.
	Highest recorded.	Mean.	Lowest recorded.	Mean.		
January . .	52·9	42·6	26·8	34·8	38·70	23·9
February .	61·9	49·4	25·3	38·7	44·05	21·7
March . .	55·2	47·0	30·0	36·4	41·70	24·1
April . . .	69·1	54·0	30·5	39·4	46·70	27·3
May . . .	61·4	52·8	32·7	41·2	47·00	30·7
June . . .	78·1	65·4	43·1	51·5	58·45	38·4
July . . .	87·8	71·3	46·6	54·0	62·65	45·2
August . .	76·9	64·2	44·3	50·8	57·50	40·1
September .	69·0	59·7	32·9	49·4	54·55	29·1
October . .	55·9	50·7	32·2	40·5	45·60	28·7
November .	57·2	47·2	29·2	39·5	43·35	24·5
December .	51·3	43·5	22·1	34·2	38·85	20·1
The Year .	87·8	53·98	22·1	42·53	48·09	20·1
Year 1884 .	87·5	57·44	22·6	44·07	50·66	23·7
Year 1883 .	82·5	54·54	20·9	42·88	48·71	19·3
Year 1882 .	78·5	55·46	21·9	43·62	49·54	20·6
Year 1881 .	86·9	55·44	12·3	42·92	49·18	5·8

MONTH.	Number of Days on which the Minimum Ground Temperature was below 32°.	Number of Days on which the Minimum Air Temperature was below 32°.	Number of Days on which the Maximum Air Temperature was below 32°.	Number of Days on which the Mean Air Temperature was below 32°.
January . .	15	12	0	2
February .	6	5	0	0
March . .	13	4	0	0
April . . .	4	4	0	0
May . . .	2	0	0	0
June . . .	0	0	0	0
July . . .	0	0	0	0
August . .	0	0	0	0
September .	1	0	0	0
October . .	3	0	0	0
November .	6	5	0	0
December .	18	10	1	4
Year 1885 .	68	40	1	6
Year 1884 .	51	19	0	1
Year 1883 .	79	40	0	6
Year 1882 .	63	26	2	7
Year 1881 .	94	60	11	24

The following table shows that the mean temperature of 1885 was rather more than one degree below that of the average of the five years 1881–85, and that nine of its months were colder than the average; only February, June, and July being warmer.

MEAN SHADE TEMPERATURES OF THE MONTHS.

	1881.	1882.	1883.	1884.	1885.	Mean of Five Years.
January	31·77	40·67	42·29	44·17	38·70	39·52
February	39·30	42·59	43·34	42·30	44·05	42·32
March	42·96	45·72	37·01	44·90	41·70	42·46
April	47·33	48·83	47·51	45·01	46·70	47·07
May	54·45	53·75	49·92	53·35	47·00	51·69
June	56·87	56·04	57·21	59·85	58·45	57·68
July	65·89	59·65	57·21	60·73	62·65	61·23
August	58·77	60·20	60·63	64·50	57·50	60·32
September	56·62	53·91	55·35	59·44	54·55	55·97
October	46·48	50·23	49·67	48·90	45·60	48·18
November	48·35	43·68	42·80	41·60	43·35	43·96
December	40·77	39·81	41·45	43·15	38·85	40·81
The Year	49·18	49·54	48·71	50·66	48·09	49·24

An Account of Mr. Aitken's Experiments on the Rigidity of Chains.

BY PROFESSOR W. RAMSAY, Ph.D.

(*Abstract of Lecture given January* 11*th*, 1886.)

THESE experiments showed the rigidity of matter in motion. A rapid rotary motion being given to an endless chain by means of a pulley over which the chain is suspended, it is seen that the chain resists any effort to change its form; and conversely, if its form be changed, it retains the altered form for some time, slowly resuming its original appearance, however, under the sustained pull of gravitation. Among other experiments, the propagation of waves, and in particular of the complicated form of wave produced by a bend or kink in the chain, was exhibited; and by supporting the chain on a smooth surface, while pressing it between two revolving pulleys, the chain was caused to stand on end, each link being shot into the air on passing between the pulleys.

On the Critical Condition of Fluids.

BY PROF. RAMSAY AND DR. S. YOUNG.

(*Abstract of Paper read March 4th*, 1886.)

IF a liquid be heated in a closed space, it evolves vapour, which exerts pressure on the walls of the containing vessel, as in the well-known case of water in the boiler of a steam-engine. At the same time the liquid expands, and thereby grows specifically lighter. If the temperature be raised, the vapour grows more dense, for pressure is rapidly increased; and when the liquid by expansion acquires a density equal to that of the vapour, rendered heavier by compression, the vapour and liquid mix, and indeed can no longer be distinguished from each other; the fluid matter, under such conditions, is homogeneous throughout. To such a state for every liquid correspond a particular temperature, pressure, and volume, and this state is named "critical." It is thus proved that there is no abrupt transition from the state of liquid to that of gas, but that the two conditions of matter are really continuous. This critical condition was exhibited with sulphuric ether, and the effect of altering temperature, pressure, and volume experimentally demonstrated.

"Sleep and Dreams."

By G. MUNRO SMITH, L.R.C.P.Lond., M.R.C.S.

February 4th, 1886.

IT is obvious that the subject I wish to call your attention to this evening is a very wide one; for the consideration of the nature and phenomena of sleep in all its forms necessitates also an investigation into the nature of life itself which is its antithesis, and death which it mysteriously resembles; and if we endeavour to follow some of the questions involved to their limits, we shall find ourselves embarked on a sea of stormy metaphysics, in which there is great danger of being hopelessly overwhelmed. To avoid this as much as possible, I am anxious to confine myself chiefly to some of the facts and theories bearing upon the *causation* of sleep. To do this with any hope of success, we must try to break into the region of poetry and romance, to rescue the subject from the fatal moonlight glamour in which it lies, and to expose it to the daylight of physiology. I do not mean to imply that this has not been done before, and I certainly do not in this paper claim any originality. Dr. Carpenter, M. Mauray, Abercrombie, Dr. Simmons, and many others have carefully investigated it in a scientific spirit, and have thrown great light on it; but most people seem inclined either to take it for granted that to go to sleep is part of their nature, and therefore needs no future investigation, or on the other hand, to leave the whole question in the hands of the poets. The fact that we lie down to sleep every night does not make it any the less extraordinary; and on the other hand mere wonder will not help us to knowledge.

It is impossible to understand the subject unless we consider it in a wide sense, and include not merely the alternations of mental rest and activity through which we pass, but also the hibernation of animals, the sleep of the vegetable world, the rest of individual cells and organs, and even, perhaps, the death-like trance of the seeds of plants which has been known to extend over thousands of years, the germ of life being quiescent but not destroyed. This is a wide range, and leads us into such far fields that we can only wander over a small part of the ground, stopping when we come across facts or thoughts that bear on the question, "What is sleep?"

We may first notice that in its broadest sense sleep, "the wide blessing," is universal amongst organised beings; it is necessary for them at some period of their existence to slacken their activities, and rest awhile to gather fresh strength. Organisms whose existence is tranquil and languid do not as a rule require such frequent and regular intervals for repose as those who live faster. Creatures of a sluggish nature, and plants whose growth is generally slow and careless, do not stand in such need of constant renovation as animals with muscles that are frequently on the move, and nervous systems that see and hear and think and feel. This may be said to be the chief function of sleep—to allow the used up force to be replaced. The repair is probably, as Herbert Spencer points out, as rapid during the day as during the night; but whilst awake the waste greatly exceeds it. During complete rest, on the other hand, repair greatly exceeds the waste. But I think it has other uses. Some animals, for instance, such as the dormouse, pass the coldest part of the year in a state of hibernation, in which condition their activities are reduced to the very lowest ebb consistent with life. Their breathing is shallow, the heart-beats are feeble and slow, and their brains and muscles are at almost absolute rest. Only now and then, when warmed by an unusually mild day, does the circulation quicken, and across the sleepy brain comes a dim vision of the nuts that the creature has laid by, and an impulse to get up and eat.

It seems to me that this is not merely a provision for recouping its energies; but it keeps the little beast in a state of security during a dangerous time of the year, when food is scarce and protecting foliage gone; when it would starve if it required as much food as in summer, and when it would be more easily captured by its enemies because it would be more easily seen than in leafy seasons. In the case of the dormouse the energies which make up life are latent, waiting for the warmth of the spring sun to act as a stimulus and awaken them. This strange phenomenon of "latency," which must be carefully considered in reference to sleep, may be illustrated by another example. Take a fowl's egg that has been laid, say, a week. Leave it alone, and in a month's time it will be about fit for the London market; leave it a year, and it will be scarcely fit for that; it dies because it does not receive the right stimulus to rouse its activities. But put this egg into a warm incubator for three weeks, and, barring accidents, presently outsteps a chick. The warmth has awakened it to life.

Look at another instance. Last August, when in Cornwall, I was told that some peas on a dinner table were the direct descendants of a few shrivelled seeds that were found enclosed in the hand of a mummy. They had grown in Egypt in the time, perhaps, of the Captivity, and had lain in their strange hiding-place ever since, in hopes, getting fainter and fainter, of a resurrection to life. They were planted in England: some grew, flowered and bore seeds, which in their turn gave rise to many descendants. It is not, I think, pushing the phrase too far to say that these seeds had been asleep. Their energies had lain dormant, not dead, and they merely wanted the right stimulation, viz., moisture and warmth, to re-animate the germ within them. In the case of the seed, the egg, and probably the dormouse, some stimulus is necessary to make the latent energy potential. There appears to me to be a great analogy between these cases and the sleep of man, which is dependent, as I hope to show, on the withdrawal of stimuli, and requires the action of stimuli to convert it into the waking state. This is at least partly true. Withdraw all stimuli, and under certain circumstances sleep will be induced. This will be made clear by an example or two. What do we usually do, for instance, when we wish to sleep. Some people can do so anywhere and at all times; they have merely to lie down, close their eyes, and they become unconscious in a few minutes. But the usual thing is to carefully withdraw, as much as is possible, everything that can excite the mind or body. We lie down, and by so doing distribute the pressure as much as we can over our surface, so that the feeling of contact is diminished, and this is aided by the softness of the bed. The attitude we assume, moreover, is that in which the muscles are most completely at rest; but the curled up position, with the knees drawn up, is partly also for the sake of warmth.

Almost all animals roll up in this way, and especially in cold weather. In fact, the more effectually we can keep off any kind of sensation from our bodies, whether roughness, cold, or pressure, the more readily we shall sleep. Then we get the room as quiet as possible, the stiller the better, and we keep out the light and close our eyes. Here again our object is to keep impressions off our nervous systems. By these means we isolate ourselves from the external world; the sights and sounds and manifold sensations of the day are withdrawn, and the only disturbing causes are stimuli from within. These are chiefly of two kinds: (1) internal pain or discomfort, the action of which in preventing sleep is obvious; and (2) the activity of the higher nervous centres, especially our thoughts, which act in much the same way as external stimuli. Each thought, if vigorous enough, engenders others, and the mind is kept in activity. This is not of itself, however, sufficient to interfere with sleep if the brain be exhausted, unless the ideas which pass through the mind are very strong or painful. The sleep of early childhood, when thoughts have not developed into tyrants, appears to be never broken by anything but bodily suffering. It is curiously regular, "full of sweet dreams, and health, and quiet breathing." Most people find by experience that it is important to direct the current of thoughts into smooth channels, and as we cannot always control this current, there are many devices for keeping it within bounds, such as counting or repeating to one's self some monotonous sentence or idea; for example, the well-known story of the sheep going one by one through a gate. Keats spoke of sleep as the "low murmurer of tender lullabies," and anything with a slowly alternating cadence or see-saw tune is very potent in producing it, and monotonous sounds, such as falling rain or the

Plate

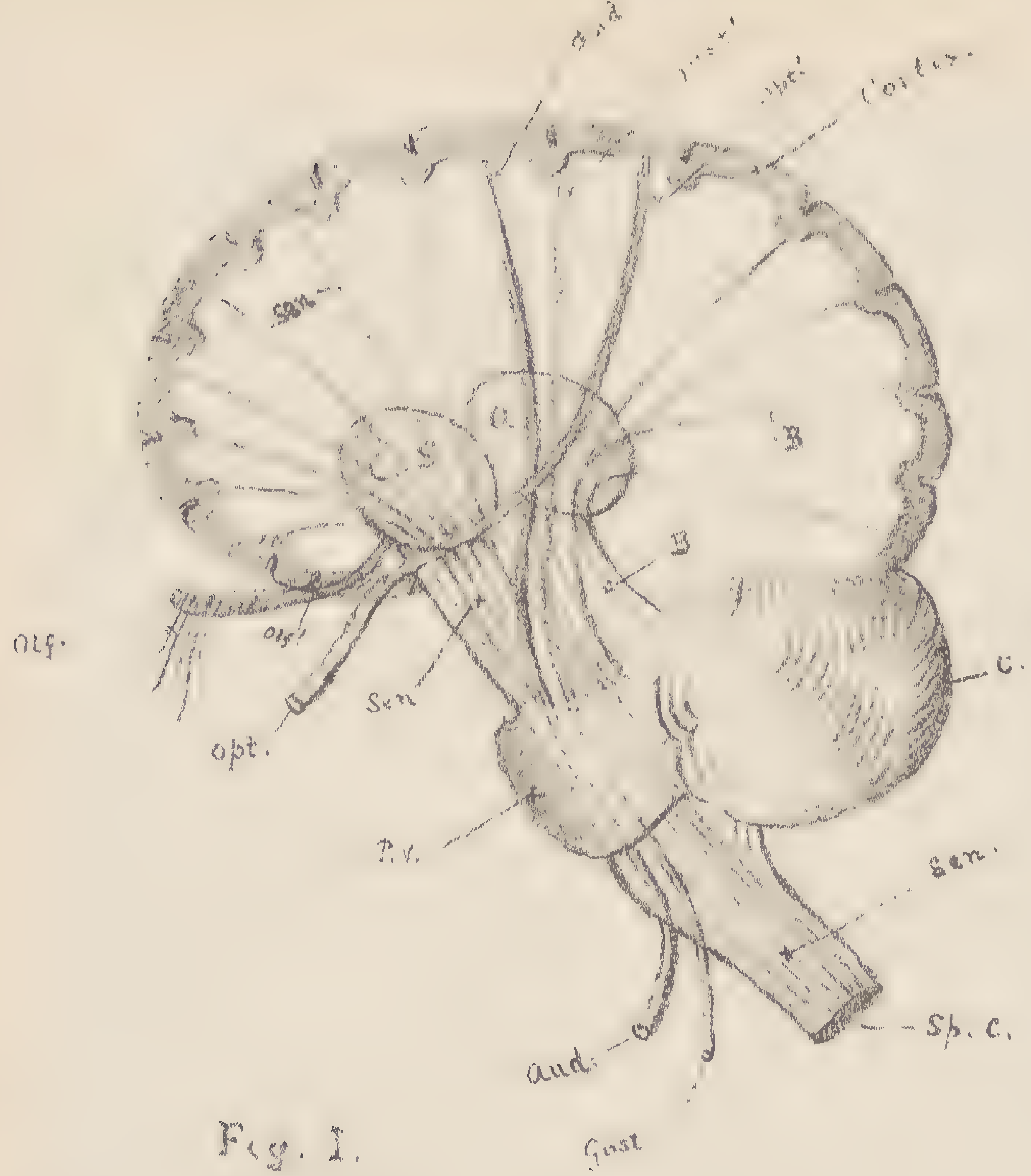

Fig. 1.

Fig 1.—Diagram to represent the channels of communication between the cerebral cortex and the external world. c—Cerebellum. sp.c.—Spinal Cord. p.v.—Pons Var lii. B.—Fibres from the Cerebellum to the Cortex, probably carrying sense of "position" as regards our surroundings. Sen.—Nerves of ordinary sensation carrying impulses from Skin, &c., to Cortex. Gust.—Nerve of Taste ending in Cortex at Gust! Aud.—Nerve of Hearing ending at Aud! Opt.—Nerve of Sight ending at Opt! Olf.—Nerves of Sense of Smell ending immediately in Cortex. C.S. & O. are two masses of Nerve Cells through which the various Fibres pass on their way to and from the Cortex. (Most of the Fibres of ordinary sensation pass through O. not C.S.

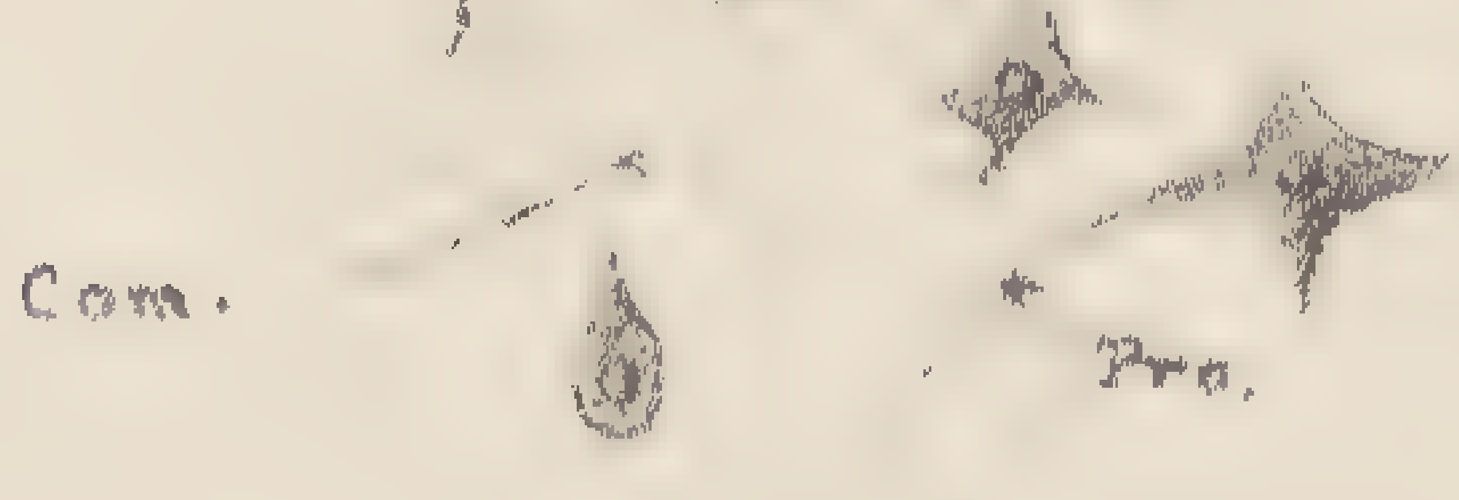

Fig. 2.

Fig 2.—Nerve Cells from Cortex of Brain. At Com is seen a communication between two of them, and at Pro a filament probably connecting the Cell with some nerve fibre.

sound of distant water. The mere reading of Tennyson's "Lotos Eaters" makes some people drowsy. Rhythmical sensations, too, act in the same way. The vampire bat is said to fan his victim to deepen his slumber, and I have been told that gently patting a baby will act in the same way. The cradle will also serve as an example of the same thing. The object in all these devices is to keep the mind as nearly as possible in a state of vacuity by substituting the very simplest mental process for the more exciting one; withdrawing the stimulation of thought.

A remarkable case was recorded by Pflüger in 1877. A boy had, from disease, lost the power of feeling over almost the whole of his body, and had become deaf in one ear and blind in one eye. He had, therefore, only two channels by which extrinsic stimuli could act upon his brain. When the sound eye and ear were closed he almost immediately fell asleep. It is a well-known fact that if you softly wrap up a frog's head in a damp cloth, it lies perfectly still and happy, its nervous system in absolute repose, because the excitations which keep it awake are removed.

The point I have been trying to keep in view hitherto is this: we possess, in common with most other animals, several distinct senses, by which we are kept in communication with the external world. This communication constitutes, in fact, our active life. Our central nervous system, which is the great recipient, is assailed and battered by sights and sounds, scents and feelings, coming from our environments, and if in good working order it reacts, and our actions and thoughts are to a great extent the result. Our very life is, in fact, more reflex than we imagine. Cut off these communications, and our outward life ceases. This may, perhaps, be made clearer by a diagram. (*Fig. 1, Plate IX.*)

After a certain number of years the cerebral cortex becomes more automatic and less dependent on stimuli from without. It lives on past perceptions; and just as walking at first requires every movement to be carefully planned, yet after a time becomes an unconscious action, so the numberless impulses carried through many years to the cortex, are stored up by it as experience and memory. I think, therefore, that what we call wakefulness is, in a great measure, the result of the action of our surroundings on our easily stimulated nerves; and other things being equal, the more excitement and activity and friction that go on around us, the more vitality will be called forth.

This theory does not, however, altogether explain matters. We are the servants of habit and the slaves of instinct. It is the law of the animal world that at some period during the twenty-four hours we should sleep, and we inherit this habit as we do others. We sleep because it is our instinct to do so, and this instinct cannot be broken through. It is a part of that rhythm which runs through nature. We have accumulated it during thousands of years; but even during the comparatively short time that man has set apart one day in the week for rest, some people have already acquired a "seventh day's resting" instinct, and can tell without an almanack when Sunday morning comes round by the indifference they feel to exertion; and there is no doubt, I suppose, that some domestic animals experience this to a certain degree. This, then, is another reason of our nocturnal rest.

Now it has been stated that the immediate cause of sleep is a bloodless condition of the brain; this, indeed, is the commonly received theory. With whom it originated I do not know, but Dr. Hammond and Mr. Durham proved pretty conclusively, from experiments on animals, that the brain is paler during sleep than when awake, and Dr. Hughlings Jackson examined the arteries of the retina of a man, and found them contracted after he had been asleep some time. The large arteries in the neck are called the *carotid*, from the verb Χαρόω, to sleep, because when they are firmly pressed, and the brain thereby deprived of blood, the subject becomes unconscious. Many of us know that one way of getting to sleep is to eat a good dinner, and then sit in an arm chair with our feet to the fire. The explanation usually given is that the dinner draws blood to one part, the fire to the other, and the amount of blood in the brain is thereby diminished.

There are, however, reasons for thinking that this condition is not the cause of sleep, but the consequence or concomitant. We sleep, and *then* our circulations become more languid, and the brain, where the flow of blood meets with many impediments, suffers with the rest of the body. There is no good ground for supposing that our brains become anæmic towards night, and there are, moreover, obstacles to the diminution of blood in the head which do not exist elsewhere in the body. There is, nevertheless, a period, ranging from about 12 p.m. to 4 or 5 a.m., during which the temperature, and probably the circulation of the body, are lowered, independently of surroundings. This is the feeblest time of the twenty-four hours, and the time when death is probably busiest. It may be compared with the other rhythmical phenomena of our bodily functions. But we do not find the depth of sleep coincident with this decrease of temperature and languid flow of blood. Direct experiment (according to Hermann) shows that it increases in intensity from the commencement: first quickly, then more slowly, till the end of the first hour, and then it gradually diminishes towards morning.

Another hypothesis as to the immediate cause of sleep is his: that as the nerves act they undergo chemical change, and the waste products of their activity accumulate during the day and clog their action. This is an insufficient explanation on various grounds, amongst others, that these waste products do not probably accumulate more in the brain than in other parts, such as the heart muscle, and there is no particular reason why they should be removed by sleep. Another undoubted reason of sleep is the most certain, viz.: the mere exhaustion of the nerve cells. No part of the body can go on for ever, and some parts require renewing or resting more often than others; the nerve cells appear to require nearly a third of their time to re-create themselves. I think we may give the three main causes of sleep, therefore, as follows:—(1) Exhaustion of the nerve cells; (2) withdrawal of those stimuli from the nerve centres which tend to keep them awake; and (3) inherited habit.

Amongst the many interesting enquiries that have occurred to me whilst thinking about the subject, I will confine myself to two or three.

(1) *As to the average quantity of sleep.* This is, to a very great extent, a question of habit. Any one who sleeps nine or ten hours out of the twenty-four, feels drowsy if suddenly obliged to take only eight. If an ordinary man accustoms himself to six or seven, he will be as active during the day as if he took eight or nine. Individual peculiarity, no doubt, makes a difference; quick brains as a rule, I think, require less than slow ones.

The accounts of saints and hermits, who only allowed themselves four or five hours a night, are quite credible; in fact, the more sleep that is taken over and above seven hours probably diminishes the activities of the daytime. It is stated that men who are capable of long continuous mental application are also able to sleep uninterruptedly for fifteen or even thirty hours; but I cannot vouch for this.

(2) *To what extent is sleep dreamless?* Before answering this we must try to get some idea as to what dreams are. It is a very obscure subject, as is to be expected; for dreams are clearly modified thoughts, and "thought" cannot be explained. But we know, from the experiments of disease and physiology, that we cannot think without a brain and the only part of the brain that feels and originates ideas and movements is the surface: what is called cortex or rind. This is composed, as we find under the microscope, of fibres of different kinds, blood-vessels, and large irregular bodies called nerve-cells. (*Fig. 2, Plate IX.*) These cells are undoubtedly the seat of thought. That is about as much as we can say. Whether they manufacture thought as a liver-cell manufactures bile, or whether they are the machinery by which thought works and manifests itself, as the locomotive is the apparatus by which the force of steam may make itself felt and seen, we cannot say.

These cells, therefore, are concerned in dreams, which are our sleeping thoughts, and it will throw light on the subject if we stop to inquire what the functions or duties of these cells are: (1) They think, or are the only organs of thought. (2) They originate movement (this function is really a kind of thought). (3) They feel. (4) They act as modifiers of other cells or other nerve impulses, converting the latter from sensation into movement, etc.; and (5) They have the power of restraining or holding in check most of their other energies. This last, which is often lost sight of, is in one way the most important of all, because without it the others would be as useless as a runaway horse. For instance, in convulsive diseases, such as tetanus, they lose the command over themselves, and originate movements at random. In madness, they lose their control, and thoughts are poured out in abundant and wild confusion. This gift of "inhibition," as it is called, is much allied to the passive form of "will," and it seems to me that in sleep its effect on thoughts is paralyzed. Our sleeping ideas are rarely orderly or consecutive. They are not linked together by cause and effect as in our waking hours. We reason, but we alter our premises in the wildest manner, and the conclusions we arrive at are ridiculous. We do not see that they are so, because the sense of humour, which I venture to define as a keen appreciation of cause and effect, is absent in sleep, or almost so.

For example, I remember dreaming that the day of judgment had come, and I was waiting in a garden with some friends to hear the awful verdict. In the path of the garden was a round trap-door. We were told that this led down to the bottomless pit, and we had to jump over it. If we succeeded, we were saved; if not, not. This dream was very vivid, and I was in mortal terror; but it never struck me that the whole thing was ludicrous. My sense of bathos was for the time completely gone. Most people have similar experiences; in fact, we are for the time mad, and our thoughts ramble at their pleasure, without their keeper—the will; and when, at waking, they are gathered into leash, and put in order, we often do not know that they have been busy at all. The workings of the mind are not corrected and altered by the impulses from the outer world, which modify their action in the daytime. The disorder of dreams seems to me capable of partial explanation on the theory I have already mentioned at some length, viz.: the withdrawal of stimuli. It is curious how we forget what we have been dreaming about. People have nightmare, scream and kick, and obviously suffer in their minds; yet if you ask them at the breakfast table what they have dreamt of, they answer, "Nothing." This is not so often the case if we try to remember the moment we wake what we have dreamt of; but even then it often requires an effort of memory to recall the dreams. By some train of association of ideas, many people do not remember the dreams of one night until they are on the point of going to sleep the next, when they suddenly come back to the mind; and sometimes trifling incidents in the daytime recall forgotten dreams. M. Mauray, in his work, "Le Sommeil et les Rêves," discusses the question why some dreams are remembered, others not, and suggests that those occurring just before waking, and partially mixed with consciousness, are those fixed on the mind. But there is no proof of this; and, indeed, there is no more reason why we should remember all our night thoughts than all our day ones. It is difficult to ascertain, therefore, to what extent sleep is dreamless; but on theoretical considerations, I think it is very rarely, if ever so. It is not likely that the functions of the brain should be in *complete* abeyance; and we know that people dream without any memory of having done so, as in chloroform and nitrous oxide sleep, in which they talk, gesticulate, laugh, swear, sing, and behave in a manner which clearly proves their thoughts are active enough, but when they come to themselves they are generally oblivious of it. I think, therefore, in ordinary sleep dreams are the rule and not the exception; but, as I have said, it is a difficult point to settle. People do not care to be experimented on either. If you creep into your friend's bedroom at four a.m., and suddenly wake him with the question, "What are you dreaming about?" he is generally rude enough to tell you to go—elsewhere; and, moreover, there is a source of fallacy in this experimental method, which I shall mention directly. That the mind is very often active in sleep is an experience of most of us, especially just as we are falling off. We often see things more clearly then, and from a more unbiassed standpoint than we do in full waking hours, and ideas flash into the mind that are arrived at in the daytime only by laborious thought. There is a classical illustration of this in the fourth chapter of the Book of Job; and the thoughts that came into his mind then were so distinct that they appeared to be uttered by a spirit that passed before him. There is another celebrated instance. One of the most extraordinary and beautiful poems in our language was composed during sleep. I refer to Coleridge's "Kubla Khan." *

The explanation I offer of these cases is, that the fancy and imagination (which appear to be connected in some way with *rapidity* of thought) are loosened from the controlling and sometimes deadening influence of the will, and are not modified by impressions of sight and sound and feeling, as in the daytime.

These instances, and there are plenty more, show that during sleep the thoughts are active enough. It is natural that people should think during the night of events that have happened in the day, and these are woven very often into our fancies. A remark made by the late Dr. Symmonds, in an article on Sleep, struck me as being good: that dreams cannot distinguish between realities and memories; and the memories become more vivid because the constant irruption of the thoughts and feelings is stopped. In the daytime concentration on one topic, by excluding these disturbing influences, causes a day-dream, which may be exceedingly vivid, and may, perhaps, account for many of the so-called apparitions that have been recorded.

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* Traill's "Life of Coleridge," p. 61.

Another very extraordinary phenomenon of sleep is this, that during the few seconds of waking a dream may take place which appears to extend over hours of active life. It is narrated of a soldier that he dreamt he had deserted the ranks, had been searched for and found, had up before a court-martial, and condemned to be shot. He had a vivid picture of the place where he was to be killed, and he awoke with the report of the guns. This report was actually a signal gun, which caused all the rest of his dream. The noise which made the dream appeared to end it and be its consummation.

Dr. Fleming compressed the carotid arteries of some men, and generally found that during the few seconds that they were asleep they had apparently hours of active dreaming. This is a marvellous illustration of rapidity of thought. The idea of time is ignored by the half-awake brain. In the first instance, of course the dream occurred *after* the gun-shot, but the mind did not distinguish between past, present, and future, and projected the incidents backwards, so to speak, instead of in their right sequence. I should like to hear a good explanation of this. It is because the method of waking may be the *cause* of dreams, that it is useless to arouse any one suddenly in order to ascertain what they are dreaming about. It has been pointed out to me, that when we hold conversations with imaginary people in our sleep, we do not know beforehand what answer we shall get, and are sometimes surprised at it, although we, of course, make the answer as well as the question ourselves. This is an instance of the disconnected condition of our thoughts, and also of the rapidity with which they change.

Certain dreams are very common; one especially, which is rarely quite the same in any two people, but is something like this: you imagine yourself to be either walking or running, and take longer and longer steps; presently you find, to your great joy, that you can, by a slight effort, keep off the ground for a long time, and finally that you can float along without touching it at all, and swim through the air in an upright or flying attitude. It sometimes takes the form of jumping, or rather floating, down flights of stairs, rarely getting to the bottom. These phenomena are probably allied to giddiness, and may be due to a too languid or disturbed circulation in the brain. Whether the dream is pleasant or not, seems to depend very much on the bodily condition. Abercrombie narrates the case of an old gentleman he knew, who, forty years before, had been pursued by an infuriated bull; since that time, if he indulged in too heavy a supper, he almost always dreamt that this beast was after him. The usual forms of nightmare can be caused in some people with great certainty, by eating certain kinds of food at night and lying on their backs. They are most likely due to sympathetic disturbance of the heart and circulation, and actually accompanied by pain or discomfort; but why they should assume the shape they do, of intense desire to escape from some horrible danger, with complete inability to do so, I cannot say. The nervous impulses which we are anxious to send to our muscles, seem incapable of going down their channels, probably because the nerve fibres lose their irritability, and do not respond. In some very vivid dreams this is overcome, and the alarmed sleeper jumps out of bed. There is a case described of an officer who could be induced to dream by whispering in his ear, and was often made to imitate the actions of swimming, and even of firing a pistol, by being told to do so. This is remarkable, as being on the border-land of mesmerism. It is noticeable that in this case he never had any memory of these violent dreams when he awoke; and the subject of a mesmeric trance has no recollection of what he has done whilst in that condition.

Another curious point is this: many people declare that they can wake at any time they like by merely deciding to do so when they go to bed. There is certainly some truth in this, but it is complicated by the fact that when the mental alarum is set for a definite hour in the morning, one is apt to wake not merely then, but several times before; in fact we are on the *qui vive* all night. Nevertheless, it is too often done correctly to admit of much doubt, and is a striking illustration, I think, of the activity of the thoughts during sleep without our retaining any memory of them when awake. I have tried this on myself, with partial success and sometimes disastrous results; for example:—

Sept. 26th. Set my "brain-clock" for 7.15, woke at 6.15, and then slept soundly until 8.45.

Sept. 28th. Set for 7, woke about 4.30, then at 6, and then not till 8, thereby losing an important engagement.

Oct. 26th. Set for 7.20, woke at 6, and then at 7.20. (*Note.*—Had no special business early, so did not worry about waking.)

Some of my friends have been more successful. A., for example set for 4 a.m., and woke at ten minutes to the hour; woke several times before. Set for 3, and woke almost exactly at 3, and did this several nights running. The same when set for 6. Set at 4, woke at 4.10. B., can always do it by tapping the required time with the fingers whilst going to sleep, etc., etc.

The phenomena of *mesmerism* throw some light on the nature of sleep. The subject mesmerised or "sensitive," as she or he is called, must either, I think, have a will that can be dominated or subjugated by a stronger, or must have the power of intense concentration on one thing, to the exclusion of all others; must in fact be able to block up all senses but one, as effectually as we do when we draw the "curtains of repose" at night. If the eyes or attention waver or wander, the experiment is a failure. It is in fact a "withdrawal of stimuli," and true mesmeric states can be explained, I think, on this hypothesis. In 1850 two itinerant Americans came to this country and produced a great sensation by mesmerising people. They made the subject of the experiment look fixedly at small copper plates which they held before the eyes, and hence they designated themselves by the ridiculous name of electro-biologists, a name which has clung ever since. Mr. Braid took up the question and soon discovered that any small object would do as well as copper, the only requisite being that the person operated upon looked steadily at it for a long enough time.

There is an immense amount of humbug clouding the subject, and it almost baffles investigation, because you can never be quite sure that the people you are dealing with will not *act* or deviate from the truth.

Mr. A. G. Leonard, of the Psychical Research Society, very kindly arranged a mesmeric *seance* for my investigation, and I think it would perhaps interest some if I narrated my experiences. I have not trusted at all to memory, but made notes at the time, many of which were seen by others present and endorsed. The *seance* was held in London, in the rooms of the above society, at 7 o'clock on the evening of Saturday, Dec. 13th, 1885. There were present the following (I give these names in full, as a guarantee to some extent of the authenticity of the proceedings): Rev. H. P. Gurney (of Wren & Gurney's), Fellow of Clare College, Cambridge, Messrs. Jebb, Charles Downing, Vladimir (a Russian artist), A. G. Leonard, B.A., Lond., W. G. Leonard, G. A. Smith, G. D. Hornblower, the sensitive, named Conway, and myself.

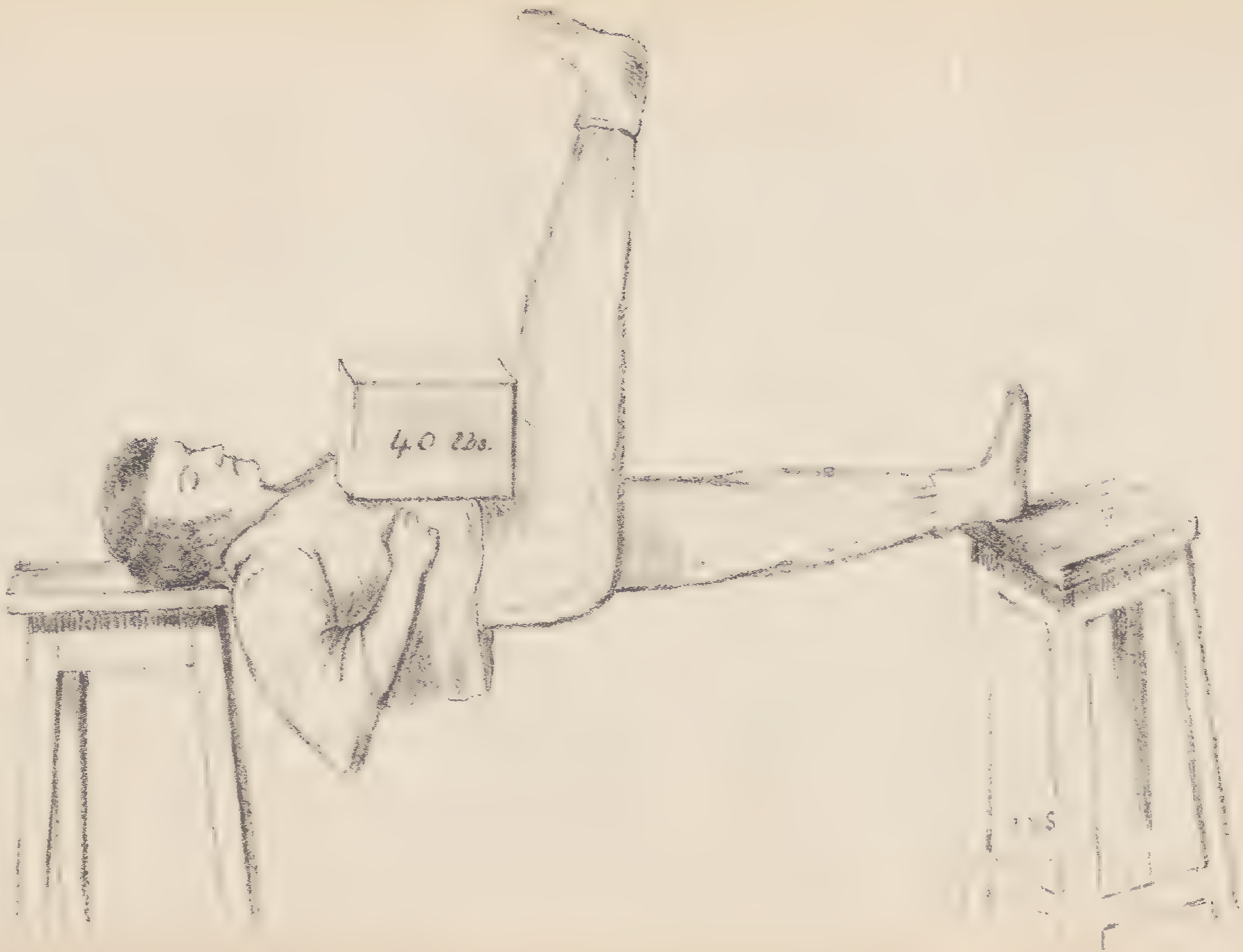
Mr. Conway, who is a spare but strongly built man of about thirty, with quiet, somewhat determined face, good colour, and dark blue eyes, deep set and rather dreamy-looking, sat in an easy chair whilst Mr. G. A. Smith made downward passes with his hands as near his face as possible. His eyes gradually closed, and in a minute or so he appeared to be asleep. Mr. Smith then told him to open his eyes, he replied that he could not; but after a few attempts and some more passes he succeeded. He was now supposed to be in the hypnotic condition, and seemed unconscious of anything in the room besides the mesmeriser, who proceeded to put him through different delusions. He told him that he (the sensitive) had a cold, and he began almost immediately to sneeze; he asked why he was so miserable, and he began to cry; what he was laughing at, and he straightway rang out peals of laughter, and so on. He acted the part of a cat, and waited near a hole for mice, scratching any one who interrupted him, and did other extraordinary things, but would not or could not hear any one but the mesmeriser, and when he was pinched or even pricked with a pin he paid no attention. When, however, I gently inserted a pin beneath his nail, he withdrew his hand. It was then suggested that what is called "community of sensation" should be tried. The mesmeriser stood behind the sensitive (who was in an arm-chair) and held his hand. The latter was then carefully blindfolded with a handkerchief. I then touched or pinched Mr. Smith in various parts of his body, and the sensitive was thereupon questioned as to whether he felt pain anywhere. He was tried in this way thirty-four times, and was right twelve times, besides once when there was some doubt; but of these twelve, five were partly capable of explanation by movement (shrinking) on the part of the person pinched, and three were when he was not being touched at all. It should also be stated, that in spite of hints to the contrary, conversation went on in the room, so that (on the supposition of "shamming"), the subject might have gathered information.

Mr. Edmund Gurney (late Fellow of Trinity College, Cambridge, Hon. Sec. to the Society for Psychical Research), arrived about 9 o'clock, and suggested that Conway should be tried with "community of taste." Accordingly Mr. Smith stood as before behind him, whilst Mr. Edmund Gurney handed him small quantities of powdered condiments and drugs. As Mr. Smith placed these on his own tongue, the sensitive was asked what he tasted. He was occasionally right, but on the whole it was not considered satisfactory.

Through the kindness of Mr. Leonard I attended another *seance*, at which the subject went through still more extraordinary pranks, but gave me the idea of "acting a part," but on this hypothesis he must have been a splendid comedian. One thing which he did appeared very strange. The mesmeriser, in this case Mr. Leonard, told him he was a Dutch clock, and he became immediately stiff and rigid, and struck the hour when told to. Whilst in this condition he was placed with his head on one chair and his heels on another. He remained in this trying posture for 4½ minutes, and resisted pressure made upon his body. His pulse was quickened, but there was no other sign of excessive muscular effort.

Dr. Cremen, of Cork, records a case of morbid somnolence in a patient who passed into a somewhat similar condition to the mesmeric one whilst in the hospital. He continued in this state sometimes for four days. When placed with his head on one stool and the heel of one foot on another, with the other leg elevated at right angles, he supported a weight

Plate X



of 40 lbs. on his chest (*Plate X.*). Dr. Cromen seems to entertain no doubt as to the perfect reliability of these symptoms. I may state that the above position can be assumed with a little practice. I have remained supported on two chairs for some minutes, and have whilst in that condition borne a man's weight for a second on my chest without bending my body.*

Mesmerism is without doubt something more than a mere fancy; Charcot's results in Paris prove this, without other testimony. It throws light, I think, on the cause of sleep, and seem explicable on the theory of concentrated attention. It requires further investigation, not so much credulity, and not so much *a priori* scoffing.

The artificial sleeps produced by opium, cannabis indica, bromides, etc., is often of an extremely agreeable character. De Quincey, the great English opium eater, has given graphic descriptions of his happy intoxication, and declares that his favourite drug possesses the keys of Paradise. In Dumas' "Monte Christo" it says, speaking of hatchis, a similar drug: "Taste of this, and the barriers of the impossible disappear, the fields of the infinite are thrown open, and you walk, free of heart and free of spirit, in the boundless realms of reverie." Opium seems to dull all the afferent nerves, all those I mean which carry impulses to the brain, but leaves the functions of the cells themselves only partially interfered with. It is difficult to say why it so often causes pleasant dreams and sensations.

In conclusion, I should like to recall the chief theories I have attempted to keep in view, viz.: 1st. That sleep is caused and required by exhaustion of the nervous system, and that this may be in some cases the chief or even the only factor. 2nd. That another factor is commonly at work, namely, the withdrawal of external stimuli, of those numberless excitations which keep us awake. I pointed out that everyday life was, to a great extent, probably the result of the action of these shocks on excitable nerve cells and fibres, and that without them life was latent, not manifested. 3rd. That our regular nocturnal sleep is an inherited instinct, following an almost universal law in being rhythmical; and 4th, That it is associated, not caused, by a languid circulation in the brain and nerve centres. Of these four I laid the greatest emphasis on the absence of stimuli, because I consider that on that theory alone can be explained many of the phenomena of trance, mesmerism, and other states.

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* Prof. Ramsay kindly asked me to a private mesmeric *seance* at University College on May 3rd, 1886. The action of the "sensitives" under Mr. Vinoro's influence was much the same as at the above.

I think that thought never quite ceases during life, and that we always dream, either faintly or vividly, but do not as a rule remember our dreams.

I have also tried to show that from the most vigorous life on the one hand, to the stillest and deepest and most death-like trance on the other, there is an uninterrupted connecting chain; that almost every state of latent energy may be looked upon as essentially the same as ordinary sleep; and that vitality is this latent energy brought into action by appropriate stimuli.

The whole subject is wrapped up in a cloud of darkness and mystery, and this is perhaps only natural, seeing that it is a state between life and death, between the "primeval light" and the "everlasting dark." But we ought, nevertheless, to investigate it by ordinary rules, and as a first step I think we should recognise its intimate connexion with other states. Since life has been defined as the passage from potentiality through actuality, so sleep may perhaps be said to be the passage from actuality to potentiality. Although, like other problems in physiology, the further it is pushed the more it baffles us, yet we ought constantly to make the attempt to master its problems, because they throw light on many other phenomena of our existence, and will continue to be food for thought until "human time itself shall close its eyelids."

Cicada Septemdecim.

By G. C. GRIFFITHS.

(*Read, March 9th, 1886, before the Entomological Section.*)

THE seventeen-years' cicada, or as it is often called in the United States, with characteristic American disregard of entomological exactness, the seventeen-years' locust, made its first recorded appearance in Plymouth, Mass., in the year 1633. "At that time," says Governor Morton, "there was a numerous company of flies, which were like for bigness unto wasps or bumblebees. They came out of little holes in the ground, and did eat up the green things, and made such a constant yelling noise as made the woods ring of them, and ready to deafen the hearers." Judge Davis, in the Appendix to his edition of Secretary Morton's "Memorial," says that these insects appeared in Plymouth, Sandwich, and Falmouth, Mass., in 1804; but if the regular seventeen-year interval had been exactly observed, this last-mentioned visitation should have occurred in 1803. No doubt seasonal irregularities may at times quicken or retard their appearance, but in nearly every case the interval appears to be exactly seventeen years. At Philadelphia their first recorded occurrence took place in 1715, which will not agree with the dates of the Plymouth visitations; the times however, appear to vary in different localities, though preserving in each the distinctive seventeen-year interval. In Alabama the cicadas appear in February or March; in Maryland and Pennsylvania in May; and in Massachusetts in the middle of June. So regular in appearance are they, that at Germantown, Pennsylvania, they are stated to have emerged in great numbers on the 25th May at four successive periods. The Rev. Ezra Shaw Goodwin, of Sandwich, Mass., writing in 1832, of the cicadas, says: "I first took notice of them in 1821, on the 17th of June, from their noise. They appeared chiefly in the forests, or in thickets of forest trees, principally oak. Their nearest distance from my dwelling cannot be far from a mile, yet at a still hour their music was distinctly heard there. On going to visit them, I found the oak trees and bushes swarming with them in a winged state. They came up out of the ground a creeping insect. Very soon after they had arrived on the surface of the earth the skin, or rather the shell of the insect, burst upon the back, and the winged insect came forth, leaving the skin or shell upon the earth. Thus these skins lay in immense numbers under the trees, entirely empty, and perfect in shape. The winged insects did not, as far as I could ascertain, eat anything; motion and propagation appeared to be the whole object of their existence. They continued about four or five weeks, and then died." The life history of *Cicada Septemdecim*, as related by Harris, appears to be briefly this: The female, after pairing, proceeds to make ready a nest for her eggs. She selects a branch of moderate size, which she clasps on both sides with her legs. Then, with her abdominal piercer, which consists of three parts—namely, two outer saws working in conjunction with a central spear-pointed borer—she makes an incision obliquely into the bark and wood in the direction of the fibre, taking care with the saws to detach little splinters of wood at one end, so as to form a cover to the perforation. The hole having been enlarged by repetition of the operation until it is sufficiently deep to contain from ten to twenty eggs, she proceeds to place the eggs in the nest in pairs, side by side, but separated by a portion of wooden fibre. When the nest is completed and filled, which takes about fifteen minutes, she removes to a little distance and commences another; it is not unusual for her to make fifteen or twenty fissures in the same limb, and one observer counted fifty nests extending along in a line, each containing fifteen or twenty eggs in two rows, and all of them apparently the work of one insect. The cicadas abound most upon the oak, but sometimes other forest trees, shrubs, and fruit trees are attacked, and at times scarcely any kinds are exempt except the fir and pine family The punctured limbs usually wither and die soon after the eggs which are placed in them are hatched. The ova are one-twelfth of an inch long, and one-sixteenth of an inch through the middle, but taper at one end to a point, and are of a pearl-white colour, showing the form of the enclosed insect before it is hatched. The young cicada when it bursts the shell is one-sixteenth of an inch long, and is of a yellowish-white colour, except the eyes and the claws of the fore-legs, which are reddish ; it is covered with little hairs. In form it is somewhat grub-like; the first pair of legs is large, shaped almost like lobster claws, and armed with strong spines beneath. On the shoulders are little prominences in place of wings, and under the breast is a long beak for suction. The little creature is very active, moving almost as quickly as an ant. After a few moments its instinct leads it to descend to the ground; but to do this it does not seek the body of the tree, but runs to the side of the limb, and deliberately loosening its hold, allows itself to fall. On reaching the ground after this perilous descent, it at once begins to burrow into the soil with its strong fore-feet. It seeks out the roots of plants, and attaches itself to the succulent fibres, which it pierces with its sharp beak. One observer says: "On removing the earth from a pear-tree which had been declining for years, without apparent cause, I found the larvæ of the cicada in countless numbers clinging to the roots of the tree, with their suckers piercing the bark, and so deep and firmly placed, that they remained hanging for half an hour after being removed from the earth. From a root a yard long and about an inch in diameter, I gathered twenty-three larvæ of various sizes from a quarter of an inch to an inch in length. They were on all the roots that grew deeper than six inches below the surface. The roots were unhealthy, and bore the appearance of external injury from small punctures." After its long subterranean residence in the larvæ state, the cicada, preparatory to entering the pupal form, gradually ascends towards the surface by means of cylindrical burrows, the sides of which, according to Dr. Potter, are cemented and varnished, so as to be waterproof. The lower portions of these burrows are filled with earthy matter, removed by the insect in its upward course; the upper portion, to the extent of six or eight inches, forms a chamber for the pupa. Here the cicada remains for several days, ascending occasionally to the mouth of the hole for warmth and air; sometimes peeping out in fine weather, but retiring to a lower depth in cold or wet weather. Its form as pupa is still somewhat grub-like, but the prominences containing the future wings are more largely developed. When the favourable moment for final emergence arrives, the insect issues from the ground during the night, and crawls up the trunk of a tree, or upon some other object to which it can fasten itself with its claws It then prepares to cast off the pupa skin, which at length splits down the back, allowing the imprisoned cicada to issue forth. The wings soon attain their full size, and become hard, and in a few hours the insect is able to fly. During several successive nights the pupa continue to come out of the ground—as many as fifteen hundred have been known to emerge from beneath one apple-tree—and the ground where the insect prevails is as full of holes as a honeycomb. The branches of the trees are so laden with cicadas, that they bend, and sometimes even break with the weight. Within a fortnight of emergence, pairing and egg deposition take place; and within six weeks the entire brood of imagos has disappeared.

Reports of Meetings.

GENERAL.

THERE have been eight General Meetings of the Society during the past Session; all held in University College, at 8 p.m.

On Thursday, October 1st, 1885, Professor C. Lloyd Morgan, F.G.S., read a paper on *Animal Intelligence*. This has been published in *Mind* (Vol. xi., No. 42, April, 1886).

On November 5th, Mr. Thomas Morgans, C.E., gave a lecture on *Local Terra-cotta Clays and Products*, illustrated by various instructive specimens of local production. He stated that the Coal Measure Clays were the most valuable of all for the manufacture of terra-cotta; and that immense deposits of these existed in the neighbourhood of Bristol. Good ware is strong, impermeable, and very durable; and prolonged exposure to weather or to the action of sea-water, etc., has remarkably little effect upon it. Articles can be made of almost any size and shape, and the colour can be varied almost infinitely by the addition of different metallic oxides, and the employment of various modifications of the baking process. The surface, also, will take a good polish.

At the meeting on December 3rd, a resolution was adopted expressing the great regret with which the Society had heard of the death of Dr. W. B. Carpenter, C.B., F.R.S., who had been an honorary member for many years. Mr. H. B. Jupp, M.A., then exhibited and described two electrical instruments, recently invented by Capt. Cardew, R.E., viz.: a Telephone adapted for use by sentries, and a Voltmeter for measuring the motive force of powerful electric currents. He then addressed the meeting on the subject of "Weather Forecasts." The various facts which have as yet been ascertained in connexion with storms, and the laws regulating the direction of the winds, were clearly stated. About 93 per cent. of the storms in this part of the world move from S.W. to N.E.: the centre of these cyclonic systems usually passing from the Atlantic, across the north-west of Ireland, and then over the middle or north of Scotland. From combined observations, taken on the western shores of these islands, it is easy to warn stations on the east coast, and in Denmark, Norway, etc., of coming storms; but to foretell with certainty the advent of disturbances on the western coast, it would be necessary to have stations of observation further west, but not so far away as Newfoundland and the mainland of America. The lecturer, therefore, advocated the mooring of vessels in mid-Atlantic, with telegraphic communication, or a plan by which all vessels going westward should be supplied with trained carrier-pigeons to despatch at intervals with meteorological reports.

On Monday, January 11th, 1886, Professor W. Ramsay, Ph.D., gave a most interesting demonstration of the experiments of Mr. John Aitken, F.R.S., Edin., on *The Rigidity of Chains while Revolving.* An abstract of this lecture will be found on page 60.

At the meeting on February 4th, Mr. G. Munro Smith, L.R.C.P., M.R.C.S., read a paper on *Sleep and Dreams*, which is printed in full on pages 62–83.

On March 4th, Professor Ramsay and Dr. Sydney Young gave a lecture on *The Critical Point of Fluids*, admirably illustrated by experiments. A short account of this will be found at page 61.

At the meeting on April 8th, Lieut.-Col. Graham, R.A., read a paper on *Gunpowder and Projectiles*. He gave an interesting *resumé* of the history of gunpowder, described the ingredients and their sources, and then gave an account of the various stages in its manufacture. Lastly he described the chief varieties of projectiles. Specimens of these, and of time- and impact-fuses, gun cotton, and the various kinds of powder, from the finest up to that in which each grain was 1 or 1½ inch in diameter, were shown.

At the last general, which was also the 24th annual, meeting, held on May 6th, the Report of the Council was read, and officers were appointed for the ensuing year. Professor H. S. Hele Shaw and Mr. A. M. Worthington, were elected honorary members of the Society. The hon. secretary requested all members and associates to do their best to make the work of the Society more widely known, and urged the desirability of encouraging the formation of local Natural History Associations, affiliated to this Society, in all towns within the area of the Bristol coal-field; so that the whole district might be more thoroughly worked, and more general (and therefore valuable) results obtained.

ARTHUR B. PROWSE,
Hon. Reporting Sec.

THE BOTANICAL SECTION.

APPENDED to this number of the Society's Transactions will be found the sixth and final Part of the *Flora of the Bristol Coal-field*, containing the Sedges, Grasses, Horsetails, and Ferns.

Saturday excursions for field work were continued as usual. On May 10th, Mr. Leipner, President of the Section delivered a lecture on the Natural History of Mosses, with particular reference to their reproduction.

J. WALTER WHITE, *Hon. Sec.*

CHEMICAL AND PHYSICAL SECTION.

SINCE the publication of the last Report, six meetings of the Section have been held, and papers have been read by the following gentlemen:—Mr. G. F. Schacht, Professor Ramsay, Dr. Young, Mr. Shenstone, Dr. Tilden, Mr. Chattock, Dr. Collie, and Mr. Cundall. On April 8th there was a joint meeting of the Section and the Society, when Colonel Graham read an interesting paper on *Gunpowder and Proectiles.*

Copies of Joule's "Scientific Works," Muir's "Principles of Chemistry," and Lunge's "Distillation of Coal Tar," have been presented to the Library by the Section.

SYDNEY YOUNG, *Hon. Sec.*

ENTOMOLOGICAL SECTION.

IN June last an excursion was taken to Dursley, and, the day being fine, many species were captured, among them being *P. alsus*, in great profusion, *P. geryon*, *A. plantaginis*, *A. subsericeata*, *B. pandalis*, and *P. phæodactylus*.

In July an excursion was taken to Ashcot Moor, near Glastonbury. Among other species taken, were *Acidulia immulata*, *E. albulata*, and larva of *N. typhæ* in the stems of reed mace, and *Casseda equestris* in the stems of mint.

At the winter meetings of the Section a large number of British and foreign species were exhibited by different members of the Section; and at the March meeting a paper was read by Mr. Griffiths on the *Life History of Cicada Septemdecim*, a North American species.

GEORGE HARDING, *Hon. Sec.*

GEOLOGICAL SECTION.

THERE have been three evening meetings of the Section, at which Professor Lloyd Morgan read papers in illustration of geological principles: (1) *On Denudation in its Destructive Aspects*; (2) *On the Processes of Reconstruction*; and (3) *On the Processes of Alteration, Consolidation, and Metamorphosis.* The papers were illustrated by diagrams, specimens, and microscopic sections. At one of these meetings the President exhibited pyramidal pieces of pennant and

Dundry oolite, obtained in certain experiments he is carrying out with the object of ascertaining the weight in tons necessary to crush a two-inch cube of the rock. He stated that pennant yielded at about twenty tons.

There have been two excursions: The first to Portishead, where the President pointed out the nature of the flexures and faults by which the present conformation of the ground and configuration of the strata have been produced. The results of his investigations in this district are put forward in the paper which will be found on page 17 of the Proceedings. The second excursion was to the Patchway section and the Cattybrook cutting, under the guidance of Mr. C. Richardson, C.E. The contortions of the Palæozoic rocks in the neighbourhood of the Cattybrook fault were pointed out. In the cutting at Patchway, Professor Lloyd Morgan and Mr. Winwood obtained characteristic specimens of the Rhœtic bone-bed. The bed is here two or three inches in thickness.

A paper by Professor Lloyd Morgan *On the Portbury and Clapton District* was taken as read, and will be found on page 1 of the Proceedings. The President will conduct an excursion to this locality in the course of the summer.

A. C. PASS, *Sec.*

INTRODUCTION.

SEVEN years have elapsed since the Botanical Section resolved to commence the compilation of a local Flora. At that time our knowledge of the plants of the district was very imperfect; but in view of the fact that the publication could only be issued by annual instalments spread over a lengthened period, it was not considered wise to defer the beginning, lest perchance the end might never be attained. Thus, under many adverse conditions, the work was undertaken and carried through; gathering, as it advanced year by year, increased solidity and completeness, in proportion to the growing extent and accuracy of our own information.

Knowing this, the reader will need no explanation of what is plainly perceptible; namely, that the later portions of the "Flora" are distinctly superior to the earlier ones, both in knowledge of the occurrence of individual species, and in fulness of detail concerning their distribution.

"Additions" to parts previously published preface each of the last four numbers, and show how rapid has been our progress towards an exact acquaintance with Bristol botany. The sum of the species given in regular sequence is 986. Besides these, seventeen are recorded in the additions, making in all the large total of 1,003 species treated as inhabitants of the Bristol Coal-field. It may eventually be found that two or three of these have been included on insufficient grounds. On the other hand, in a district so extensive, and, in some portions, so imperfectly explored, we may anticipate that a few plants still remain to be discovered, as well as many additional facts to be recorded regarding the distribution of those already met with.

Our meagre botanical literature has been made use of to its full extent, as follows: two or three local references in the works of Hudson and Ray; a few pamphlets and manuscripts; some notices of Bristol plants, published chiefly in the *Phytologist;* and Swete's "Flora Bristoliensis," issued in 1854. Of the MS., a catalogue of Somersetshire plants by W. Sole, in the possession of Mr. T. B. Flower, is the most important. The notices in periodicals were mostly written by Mr. Flower, whose acquaintance with Bristol botany covers a period of half a century. He supplied a very large number of the localities given in Swete's "Flora," and to him we also are much indebted for many interesting and valuable communications. Withering's "Arrangement" contains many references to the vicinity of Bristol, but they are little to be relied on, and we have not thought it advisable to quote them, unless when confirmed by recent observation.

The Herbarium of the late Miss Powell, which is housed in the Bristol Museum, and that of the late Dr. Stephens, now the property of our Society, are excellent collections of local plants, made by trustworthy botanists. In them we have found specimens of great value, often corroborating book records which, in the absence of confirmation, might have been deemed unreliable and worthless; and in some instances furnishing examples of plants that cannot now be found, and may since have become extinct. These collections also possess the additional interest that they supplied a considerable proportion of the localities mentioned by Swete, who, as his pages show, relied almost entirely upon the three botanists we have named. It is only to be regretted that many of Dr. Stephens' specimens lack essential particulars of date or place of collection, without which such witnesses can render little service.

The Rev. R. P. Murray has very liberally furnished us with notes taken from his researches among the plants of Somersetshire; and we are very grateful to several other naturalists outside our ranks who have cheerfully contributed their labour.

Our chief aim in the construction of this "Flora," undoubtedly has been to use such care and precision as should engender confidence in its accuracy on the part of those who may have occasion to consult it. It is accordingly the Editor's intention to at once begin revising the earlier Parts, with a view to the preparation of a second edition. To that end we now venture to address a request to the friends who have already favoured us, as well as to our own members, that they will aid in making the work as complete and accurate as possible. The correction of any errors which may exist, the confirmation of stations resting on old or doubtful authority, and additional plants or stations from any part of the district, will all be very gladly received, especially when accompanied by specimens. Supported by such aid, we may not unreasonably expect to lay before the Society in our next effort a thoroughly worthy account of the botanical wealth of Bristol.

June, 1886.

Flora of the Bristol Coal-Field.

ADDITIONS TO PART I.

113.* Dianthus deltoides, *L. Maiden Pink.*

Native; fairly abundant in a pasture between Brislington and Keynsham, S. One plant has lilac petals; and a small patch, with pure white flowers and very pale foliage, may be *D. Glaucus*, L. First noticed by Mr. David Fry, in May, 1886. The plant is recorded for North Somerset in *Topogr. Botany*, on the authority of Dr. Thwaites, but his locality remained unknown. Very possibly Mr. Fry has rediscovered it.

Moenchia erecta, Sm. in Somerset. See page 32. Prior to May, 1886, our claim to possess this little plant rested entirely upon the specimens contained in the Stephens' Herbarium. As far as we knew no one living had seen it growing in the Bristol district. We were therefore extremely glad to receive from Mr. David Fry a specimen which he had gathered on the coal-measures a short distance from Keynsham. Shortly afterwards we examined the locality, and found the plant distributed in tolerable quantity over a rather limited area. It was associated with *Trifolium subterraneum*, *T. filiforme*, *Ornithopus perpusillus*, *Myosotis versicolor*, *Aira prœcox*, and, to our great satisfaction, with *Scleranthus annuus*. One of Dr. Stephens' records is thus confirmed; and on consideration of the situation and surroundings of the plant at Keynsham, we have no doubt whatever that *Moenchia* did formerly grow on Brandon Hill, the other station mentioned by him. It may indeed lurk there still, though we fear it has been trampled out of existence.

There are some interesting facts connected with the species found in association with the *Moenchia*. They are all scarce plants in the vicinity of Bristol, to be found for the most part in small quantities in a very few places, and on the same geological formation. But whether on Brandon Hill, at Clevedon, at Mangotsfield, or in the Keynsham locality, they are ever companions, sharing in fellowship the barren and scanty soil upon which alone they seem able to maintain themselves. It may be that these plants are too weak and tiny to exist among more robust vegetation, and that the force of species competition is powerful in restricting them to the spots they occupy. It certainly does not appear that dry and sterile habitats are those best suited for the growth of the plants in question. We have seen *Scleranthus annuus* flourishing most luxuriantly as a weed on cultivated land, where it was taking full advantage of depth of soil and elbow-room. But when at large in the world, and left without favour to carry on an unequal competition with the crowd of larger species, the result in our experience is that it becomes banished to spots bare of, or unsuited to, most other phanerogams.

Scleranthus annuus, L., in Somerset. On the coal-measures near Keynsham; not very plentiful. See page 33.

FLORA
OF THE
BRISTOL COAL-FIELD.

EDITED (FOR THE BRISTOL NATURALISTS' SOCIETY) BY

JAMES WALTER WHITE,

Hon. Secretary of the Botanical Section.

"*Rerum cognoscere causas.*"—VIRGIL.

PART VI.
GLUMIFERÆ, GYMNOSPERMÆ ET CRYPTOGAMEÆ VASCULARES.

BRISTOL:
PRINTED FOR THE SOCIETY.
1886.

PHANEROGAMIA.

Class 2. *MONOCOTYLEDONES.*
Div. 3. *GLUMIFERÆ.*

CYPERACEÆ.

CYPERUS. *Linn.*

817. C. longus. *L.*

Native at Walton-in-Gordano, S.

This rare and beautiful sedge has grown from time immemorial in a small plot of very wet marshy ground, believed to have been anciently a fish-pond, and situate behind some cottages in the upper part of the village. Sole, in a MS. dated 1782, says of it, "Abundantly in a pond at Walton-in-Gordano, near Possit, Somerset, a village belonging to Sir Abraham Elton" (Possit = Portishead). The plant continued to be plentiful until 1882, when the occupier of the land ploughed it and planted potatoes. At the end of August, 1883, we found many stems coming up by the sides of two ditches which intersect the field, and also among the crop; but in consequence of the disturbance their development was much retarded, and flowering delayed nearly two months. Since that time, owing to the drainage and cultivation, the sedge has become reduced in quantity, and the stems produced in successive seasons have failed to come to maturity, whence it is to be feared that *Cyperus longus* will soon be numbered among the lost rarities of the district. VIII. IX.

SCHŒNUS, *Linn.*

818. **S. nigricans,** *L.*

Native; formerly on the coast between Clevedon and Portishead, but now extinct.

"Clevedon"; *Herb. Stephens.* "By the side of a fresh-water spring which bubbles forth from amid the bosom of the rocks, was *Schœnus nigricans*, brown and muddy from the tide washing over it." *Mr. Leo H. Grindon: Phytol.* vol. i. 566. Dr. Stephens' specimen probably came from the spot described by Mr. Grindon; a place on the coast towards Portishead, where we are satisfied the plant no longer grows.

Mr. Grindon's discovery of *Schœnus*, as narrated above, forms part of a pleasantly-written account of a botanical ramble from Bristol to Clevedon, and thence to Portishead, on July 6, 1842. In this day's work, the botanist was fortunate, as besides the sedge, he records *Phleum arenarium* at Tickenham, and *Calamagrostis Epigeios* at Clevedon. All of these plants are now unknown at the places named.

(*Cladium Mariscus*, R. Br. "Wedmore and Burtle Moor, Somerset." *W. Sole, MS.* 1782. Requires confirmation.)

RHYNCHOSPORA, *Vahl.*

819. R. alba, *Vahl.*

Native; in turfy bogs, very local.

S. About the boggy sources of streams on Mendip at Blackdown. Plentiful in peat-moors on the southern border of the district. VII. VIII.

820. R. fusca, *Sm.*

Native; on peat-moors, perhaps now extinct.

S. "Burtle Moor, near Mark." *W. Sole, MS.* 1782. See also remarks by Mr. Thos. Clark in his *Catalogue of the Rarer Plants of the Turf Moors of Somerset.*

ELEOCHARIS, *R. Br.*

821. E. palustris, *R. Br.*

Native; in marshy places, ditches, and about ponds. Common. VI. VII.

822. E. multicaulis, *Sm.*

Native; in marshy places, less frequent than the last. VII.

SCIRPUS. *Linn.*

823. S. maritimus, *L.*

Native; plentiful in brackish ditches and salt marshes, by the sides of the Channel and tidal rivers, sometimes occurring two or three miles inland. VI. VII.

824. S. sylvaticus, *L.*

Native; in damp meadows, rare.

G. "Boiling Well!" Mangotsfield.

S. Abundant in a meadow under Highbury Hill, near Hallatrow. A marshy field near Wells, *Miss Livett.* "In wet places, frequent;" *Fl. Bathon.* VII.

825. S. lacustris, *L.*

Native; in deep water, rare.

G. Baptist Mills. In the river Avon here and there sparingly; at Hanham Mill dam there is a larger quantity. VI. VII.

826. S. cæspitosus, *L.*

Native; in boggy and heathy places, local.

S. Plentiful on Mendip at Blackdown and about the Mineries. On the peat-moors in the south. VII. VIII.

(*S. fluitans*, L. "Ditches frequent." *Swete, Fl.* 84. Error.)

827. S. setaceus, *L.*

Native; in marshes and on the borders of streamlets, not common.

G. Boiling Well; *Herb. Stephens.* Stapleton; *Mr. W. E. Green.* By a little stream near Mangotsfield Station. Damp wood between Charfield and Tortworth.

S. Marshy spot close to the iron works at Ashton Gate. Clevedon. Yatton Weston-in-Gordano. VI. VII

BLYSMUS, *Panz.*

828. B. compressus, *Panz.*

Native; probably extinct.

G. Mill dam, Stapleton; *Herb. Stephens.* Stapleton, *Mr. G. H. K. Thwaites.*

S. In Claverton Wood, *Dr. Davis. Fl. Bathon.* From a remark of Mr. Hewett Watson, it would appear that the Bath record was subsequently confirmed by Mr. R. Withers.

ERIOPHORUM, *Linn.*

829. **E. vaginatum,** *L.* *Hare's-tail Cotton Grass.*

Native; in bogs, local.

S. Plentiful in bogs on Mendip about the Mineries, and very abundant in another bog near the Miners' Arms Inn, also on Mendip. Peat-moor on the southern border of the district. V. VI.

830. **E. polystachion,** *L.* *Common Cotton Grass.*

Native; in bogs and marshes; of wider distribution than the last.

S. In all the bogs on Mendip, including the swampy springs on the slopes of Blackdown. East Harptree. Pensford. Yatton. Peat-moors in the south. V. VI.

CAREX, *Linn.*

831. **C. pulicaris,** *L.*

Native; in boggy and damp heathy places; also on commons among long grass.

G. Alveston. Abundant in some spots on Clifton and Durdham Downs; close to the Zoological Gardens, and on the upper slopes of the Gully.

S. On Leigh Down, near the reservoir. By the stream between the Tanpits and Failand Farm. In the fir plantation towards the upper end of Cheddar Gorge. On Blackdown, near the Mineries. King's Wood, Yatton. V. VI.

(*C. Davalliana*, Sm. "Lansdown, on the slope of a hill on which there is a clump of firs, about 1¼ mile from Bath (Mr. E. Forster);" *Fl. Bathon.* Now lost.)

832. **C. disticha**, *Huds.* *C. intermedia*, Good.

Native; in boggy pastures, rare.

G. Filton Meads.

S. Near Hampton Rocks, Bath; *Mr. T. B. Flower.* Plentiful in some marshy pastures near Draycot Peat-moors in the south. V. VI.

833. **C. arenaria**, *L.*

Native; abundant on the sandy shore of the Bristol Channel, particularly at Kewstoke, Weston-super-Mare, and Burnham. VI.

834. **C. divisa**, *Huds.*

Native; in marshes, very rare.

S. "In considerable abundance on Burtle turf-moor, near the sea-coast." *R. Withers*, 1850. Label in *Herb. Watson.* Near Kewstoke, *Mr. T. F. Perkins.*

835. **C. vulpina**, *L.*

Native; in wet, shady places, and on ditch-banks. Common and generally distributed. VI. VII.

836. **C. muricata**, *L.*

Native; in damp hedge-banks and the borders of pastures, rather common.

G. Plentiful in the lane leading from Sneyd Park towards Sea Mills. In plenty near the bank of Avon above Sea Mills. Peaty pastures in Filton Meads. Pur Down. Stapleton; *Herb. Stephens.* Tortworth. Woods above Wotton-under-Edge.

S. Bishport. Clevedon. Combe Hay. Great Elm. Sidcot. Shipham. Winterhead. Plentiful about Wells. Weston-super-Mare. VI. VII.

837. C. divulsa, *Good.*

Native; on grassy banks, rather common.

G. In several places about Coombe Dingle. In a grassy hollow (old quarrying) on Durdham Down, 1884. Plentiful about Charfield and at Damory Bridge. Abundant on some damp hedge-banks between Thornbury and Aust. Shirehampton. Wotton-under-Edge.

S. Leigh Woods. Clevedon. Churchill. Congresbury. Between Cleeve and Yatton. Great Elm. About the Tanpits and on Failand Farm. Keynsham. Norton Malreward. Portishead. West Harptree. Whitchurch. Yatton. V. VI.

838. C. paniculata, *L.*

Native; in wet and boggy places, rare and local.

S. Ditch-banks near Axbridge. Bog in the valley near Winscombe. Ditch-banks south of Wedmore. Bogs on Mendip at the Mineries. In very large tussocks abundant on both banks of the canal near Radford and Camerton. VI. VII.

(*C. teretiuscula*, Good. The old records in Swete's Flora and elsewhere much need verification.)

839. C. axillaris, *Good.*

Native; very rare.

G. It grows plentifully in one spot between Charfield Station and the gates of Tortworth Park, where it was first noticed by Mr. W. B. Waterfall in 1882.

S. Cheddar, 1883; *Mr. Richards.* Several stations a little outside our district are mentioned in *Add. Fl. Bathon.* VI.

840. C. remota, *L.*

Native; in moist woods and on hedge-banks. Abundant in many places a few miles from Bristol, but is not seen very frequently close by the city.

G. Aust. Very plentiful about Berkeley, and also near Tortworth. Near the Duchess Ponds at Stapleton. Henbury. Near Filton. Littleton-on-Severn. Sodbury. Thornbury. Winterbourne.

S. Bishport. Churchill. Clevedon. Border of a field near the Tanpits under Failand Hill. Leigh Wood. Near Hallatrow. Portbury. St. Ann's Wood, Brislington. Woodborough and Winscombe. In plenty between Yatton and Clevedon. Bishop's Wood and Knowle, near Wells. Easton. Asham Wood. Whatley. Wrington. VI.

841. C. stellulata, *Good.*

Native; in bogs and swamps, frequent.

G. Near Damory Bridge. Marshy spots by Mangotsfield Common. Shirehampton; *Herb. Stephens.* Stapleton; *Swete, Fl.* 85. Yate Common.

S. Abundant on the swampy margin of the stream between the Tanpits and Failand Farm. Blackdown. Cheddar. Downhead Common. Upper Knowle. The Mineries. Boggy fields below Winscombe. Walton-in-Gordano. Wells. Yatton. V. VI.

842. C. ovalis, *Good.*

Native; on commons and in damp pastures, frequent.

G. Formerly on Durdham Down; *Mr. G. H. K. Thwaites.* Berkeley. Mangotsfield Common. Tortworth. Yate Common. Near Stapleton; *Mr. W. E. Green.*

S. Damp places in many of the rough pastures on the Mendip Hills. By the stream below Failand Farm. Walton Drove, near Clevedon. Upper Knowle; *Rev. W. H. Painter.* Wedmore. Wrington. VI. VII.

438. C. vulgaris, *Fries.*

Native; in marshes and wet places, frequent.

G. Marsh near Baptist Mills. Filton Meads. Charlton. Stoke Gifford. Yate Common.

S. Boggy pastures under Dundry Hill, near the reservoirs. Bedminster Meads. Blackdown, on Mendip. Clevedon. Marshy pastures in the lowlands between Cheddar and Draycot. By the stream below Failand Farm. Radford. Timsbury. Stanton Prior. V. VI.

844. C. pallescens, *L.*

Native; in damp woods and pastures, rare.

G. Copse between Horfield and Filton; *Dr. H. O. Stephens.* Berwick Wood, near Henbury, June, 1883.

S. Sparingly in a marshy pasture under the western slope of Dundry Hill, May 30, 1880. One or two plants in an open glade in Leigh Wood; *Mr. E. Wheeler.* Peaty meadow below Winscombe; *Mr. W. E. Green.* V. VI.

845. C. panicea, *L.*

Native; in meadows and marshes, and also on limestone hills. Common and well distributed.

G. Plentiful in Black Rock Gully, and in one or two other spots on Durdham Down (limestone). Ashley Hill. Alveston. Charlton. Filton. Horfield. Mangotsfield. Thornbury. Yate Common.

S. Among the heath and in long grass on Leigh Down (limestone). Barrow Gurney, and the meadows under the western slope of Dundry Hill. Bedminster Meads. Common about Blackdown on Mendip, and elsewhere in that district. Between the Tanpits and Failand Farm. Walton Drove, and in other places near Clevedon. Bogs on Mendip towards Wells. Woodborough. Yatton. V. VI.

A curious variety or monstrosity of *C. panicea* was met with near Bristol some years ago. It had double perigynia, the second or upper one with its peduncle passing through the orifice in the lower one.

(*C. limosa*, L., has been recorded from the bogs on Mendip; but probably in error.)

846. C. strigosa, *Huds.*

Native; in ditches and wet places, rare.

G. Llewellyn's Wood, Westbury; *Dr. H. O. Stephens.* Lanewood and Shortwood, Pucklechurch; *Withering*, II. 97.

S. Ditch-bank by the side of a lane between Cleeve and Yatton; first reported by Miss Winter. The plant grows here plentifully in a patch some yards in extent. Bishop's Wood, Wells; *Rev. E. S. Marshall*, 1883. Banks of the brook, Nailsea; *Herb. Stephens.* Wraxall; *Dr. H. O. Stephens.* "In woods at Charlcombe and Claverton, *Dr. Davis*"; *Fl. Bathon.* V. VI.

847. C. pendula, *Huds.*

Native; in moist woods and damp, shady hedge-banks, rather common.

G. Baptist Mills. In many places between Almondsbury and Compton Greenfield. Abundant about Berkeley. Charfield. Tortworth. Plentiful in Berwick Wood, Henbury.

S. Bishport, and in several other places under Dundry Hill. St. John's Lane, Bedminster. Long Ashton. Leigh Woods. Stockwood. Whitchurch. Canal Bank, and elsewhere near Monckton Combe. Farrington Gurney. Stone Easton. Paul Wood, near Temple Cloud. Borders of streams near Wells; *Miss Livett.* Wrington. Yatton.

V. VI.

848. C. humilis, *Leyss.* (*C. clandestina*, Good.)

Native; on downs and limestone hills, very rare and local. It is seldom found in level turf, but prefers steep, rocky banks, and the edges of old excavations.

G. St. Vincent's Rocks and Clifton and Durdham Downs, plentiful in some spots. Not abundant above the Suspension Bridge, although it is scattered over the slopes towards the Zigzag, and more sparingly between the Zigzag and Ghyston House.

S. Limestone slopes under Leigh Wood, rather scarce. Abundant on Brean Down. IV.

849. C. digitata, *L.*

Native; in a wood and thickets on limestone, now confined to a very small area.

G. Amid the underwood on St. Vincent's Rocks; usually closely concealed in long grass beneath the bushes, and needing sharp eyes to detect it.

S. Mossy ledges and recesses in Leigh Wood, where the rock is damp and shaded. Here the plants are larger and more numerous than on the Gloucestershire rocks opposite. IV. V.

850. C. præcox, *Jacq.*

Native; on downs, dry banks, and pastures, common, and generally distributed. Tall specimens are sometimes found in marshy fields, which in habit much resemble the next species. IV. V.

851. C. pilulifera, *L.*

Native; on downs, heaths, and commons, probably more frequent than our records show.

G. Clifton and Durdham Downs, growing chiefly in the furzy spots among coarse grasses. Blaise Castle Wood; *Herb. Powell.*

S. Downhead Common. Wells. V. VI.

852. C. glauca, *Scop.*

Native; in meadows and pastures, on downs and about rocks. Very common and universally distributed. V. VI.

853. C. flava, *L.*

Native; in marshes and boggy places, frequent.

It is probable that all our plants belong to var. β *C. lepidocarpa,* Tausch, as we have not yet met with any *eu-flava genuina,* Syme.

G. Wet places about Mangotsfield Common. Yate Common. Formerly on Durdham Down and at Shirehampton; *Swete, Fl.* 85.

S. On the swampy margin of the stream between the Tanpits and Failand Farm. Boggy sources of streams on Blackdown and elsewhere on the Mendip Hills. Cheddar. The Mineries. Shipham. Walton-in-Gordano. V. VI.

854. C. extensa, *Good.*

Native; in salt marshes, very rare.

S. Sandy marsh on the coast at Berrow. Weston-super-Mare; *Herb. Stephens.* VI.

855. C. distans, *L.*

Native; in meadows and marshes, frequent.

G. Abundant in the meadows by the Avon below Cook's Folly. Baptist Mills. By the Severn near Berkeley, and elsewhere in the lowlands lower down. Chipping Sodbury. Plentiful in Filton Meads.

S. Bedminster Meads. Barrow Gurney. Chew Magna. Great Elm. Highbridge. Uphill. Walton-in-Gordano. Yatton. V. VI.

856. C. binervis, *Sm.*

Native; on moors and commons, rather rare and local.

G. Blaise Castle; *Swete, Fl.* 85. Horfield, and Boiling Well; *Herb. Stephens.* Yate Common.

S. Downhead Common. Abundant on Blackdown, and at the Mineries on Mendip. VI. VII.

857. C. depauperata, *Good.*

Native. We understand that this has only been gathered twice, and is likely to be now extinct.

S. "Axbridge (Norman)"; *Herb. Watson.* Wood near Axbridge; *Mr. T. B. Flower*, who sent specimens to the British Museum, and states that the locality has been destroyed.

858. C. sylvatica, *Huds.*

Native; in woods, common throughout the district.

V. VI.

859. C. Pseudo-cyperus, *L.*

Native; on the margins of ponds and ditches. Frequent in the southern portion of the district; very rare in the northern.

G. Winterbourne; *Mr. T. B. Flower*, *Phytol.* I. 328.

S. In many places in the lowlands from Clevedon to Ken and Yatton. Very abundant by Walton Drove. Pondside, Woodborough. The Watchets, near Wells. Ditch-banks south of Wedmore towards the peat-moors, where this sedge becomes extremely common. VI. VII.

860. C. hirta, *L.*

Native; in wet pastures, frequent.

G. Alveston. Charlton. Cromhall. Baptist Mills Chipping Sodbury. Charfield. Patchway. Stapleton. Tortworth. Tytherington. Wotton-under-Edge. Yate.

S. Pastures by the stream at Bishport. Crox Top. Brean. Plentiful in peaty fields near Barrow Gurney. Failand Farm. Walton-in-Gordano. Wells. Winscombe. IV. V.

861. C. ampullacea, *Good.*

Native; in bogs and marshes, rare.

S. Clevedon; *Mr. E. Wheeler.* Bogs at the Mineries on Mendip; *Rev. R. P. Murray.* Abundant on the peat-moors in the south. VI.

862. C. paludosa, *Good.*

Native; in ditches and marshes, frequent. It is very likely to be more widely distributed than appears from our notes.

G. Marsh at Baptist Mills. By the Frome near Stapleton.

S. Clevedon. Keynsham; *Mr. D. Fry.* Flax Bourton. Monckton Combe. About Wells! *Miss Livett.* Yatton! *Miss Winter.* Common near Bath; *Fl. Bathon.* V. VI.

863. **C. riparia,** *Curt.*

Native; in water, and by the sides of rivers and streams, frequent.

G. Alveston. Aust. Elburton. Filton. Siston. Marsh at Baptist Mills. Glen Frome. Tortworth.

S. Bank of Avon under Leigh Wood. Clevedon Portbury. Long Ashton. Brislington. By the Avon at Keynsham, and higher up the river. Marsh ditches in the Cheddar Valley. V. VI.

GRAMINEÆ.

(*Echinochloa Crus-galli,* Beauv. This large coarse grass appeared in 1883 upon the dredgings from the bed of the Avon and from the Float, which had been deposited the year before in the Black Rock Quarry. In 1884 there were eight or nine fine plants. Like many other aliens which sprang up at the same time and place, it will perhaps continue for a short time, and then disappear.)

(*Panicum miliaceum,* L. Casual. Seven or eight plants on dredgings deposited in the Black Rock Quarry, 1883 and 1884.)

SETARIA, *Pal. de Beauv.*

864. **S. viridis,** *Beauv.*

Colonist. A weed in cultivated fields, gardens and waste ground, rather rare.

G. In turnip fields by the Avon, at Hanham, 1882.

Near Stapleton Mill, 1883. St. Philip's Marsh. On dredgings in the Black Rock Quarry, 1883 and 1884.

S. Bank of Avon near Rownham Ferry. On the railway near Paulton, 1881. VII. VIII.

(*S. glauca*, Beauv. Casual. Several plants on dredgings in the Black Rock Quarry, 1884.)

PHALARIS, *Linn.*

865. **P. canariensis**, *Canary-grass.*

Alien; in waste places, roadsides and occasionally in cornfields, frequent. It may always be found on the rubbish in St. Philip's Marsh, and about the floating harbours in Bristol, in company with other waifs and strays from town and trade. VII.

866. **P. arundinacea**, *L.*

Native; in and by water, common.

G. Bank of Avon as far down as Crew's Hole. Between Redland and Horfield. Berkeley. Charfield. Charlton. Ditches near Henbury. Glen Frome, Stapleton. Westbury-on-Trym.

S. Ashton Gate, near the iron works. Bank of Avon, abundant from Bath downwards. Lock's Mills, near Bedminster. Camerton. Paulton. Portbury. Portishead. Walton-in-Gordano. Croscombe and Knowle, near Wells. Winscombe. Weston-super-Mare. Marsh ditches throughout the Cheddar Valley. Brent. Burnham. VI. VII.

ANTHOXANTHUM, *Linn.*

867. **A. odoratum**, *L.* *Sweet-scented Vernal Grass.*

Native; in meadows and pastures. Very common, and generally distributed. V. VI.

PHLEUM, *Linn.*

868. P. arenarium, *L.*

Native; on the sands of the Channel shore between Burnham and Weston-super-Mare; also on Kewstoke sands. VI. VII.

(*P. asperum*, Jacq. "Habitat in pratis, infra King's Weston prope Bristolium;" *Huds. Fl. Anglica.* Swete gives it as having grown near Kingsweston Inn, in 1845. These were probably ballast plants, as would be also the *Trifolium resupinatum* formerly found in the same locality. They have since been searched for many times in vain.)

869. P. pratense, *L. Timothy-grass.*

Native; in meadows and pastures, everywhere common. The slightly tuberous form (*P. nodosum*, L.) is not unfrequent on dry hills. VI.

ALOPECURUS, *Linn.*

870. A. pratensis, *L. Fox-tail Grass.*

Native; in meadows and pastures, very common and abundant on all rich land. V. VI.

871. A. geniculatus, *L.*

Native; in marshes and on edges of ponds and wet ditches, common and generally distributed. VI. VII.

872. A. bulbosus, *L.*

Native; in salt marshes, very local.

G. Pastures adjoining the Avon below Bristol, extremely abundant near Shirehampton. VI.

873. A. agrestis, *L.*

Colonist; in cultivated fields and in waste places, often on the lias clays, frequent.

G. Bank of Avon under the Downs. St. Philip's Marsh. Cornfields at Lawrence Weston.

S. Permanent and plentiful in cornfields about Bishport. Knowle. Whitchurch. Abundant in cornfields near Portishead. Worle Hill. Wells. Frequent about Bath. IV.—VII.

NARDUS, *Linn.*

874. N. stricta, *L.*

Native; on wet heaths and commons, rather rare.

G. Mangotsfield. Warmley. Yate Common.

S. Sparingly among the sedges on the boggy margin of the stream between the Tanpits and Failand Farm. Bogs on Blackdown. Frequent on Mendip near the Mineries. VI. VII.

MILIUM, *Linn.*

875. M. effusum, *L.*

Native; in woods, frequent.

G. Almondsbury. Aust. Henbury. Patchway. Between Stoke Bishop and Shirehampton. Wooded banks in Glen Frome, Stapleton.

S. Leigh Wood. Stockwood. Abundant in woods at Portishead. Clevedon. Barrow Gurney. Chew Magna. Maes Knoll. Woods around Pensford. Bishop's Wood, Wells. Wraxall. VI.

PHRAGMITES, *Trin.*

876. P. communis, *Trin.* *Reed.*

Native; in wet ditches, and by the sides of rivers and ponds. Common, especially in the extensive marshlands of the district. VII. VIII.

PSAMMA, *Pal. de Beauv.*

877. **P. arenaria,** *R. & S.* *Mat Grass.*

Native; in loose sea-sands, abundant on the coast of North Somerset. Its great importance as a means of restraining the drifting sand is recognised among the huge sand-hills towards Berrow and Burnham, where this grass is systematically planted in situations needing its binding agency. Were it not for its powerful influence in building up suitable barriers against the advance of the shore sand, impelled by the prevailing west winds, a large tract of land now under cultivation would be rendered desert and useless. VII. VIII.

CALAMAGROSTIS, *Adans.*

878. **C. epigeios,** *Roth.*

Native; in thickets and shady places, very rare.

S. Border of Bishop's Wood, Wells; *Miss Livett.* By the roadside between the Monument and Tracy Park; *Fl. Bathon.* It has been reported to grow also at Clevedon, Hutton, and elsewhere, but upon insufficient authority. VII.

AGROSTIS, *Linn.*

879. **A. canina,** *L.*

Native; on downs, heaths, and commons, frequent.

G. Clifton Down. Filton. Mangotsfield Common. Stapleton. Yate Common.

S. Backwell. Blackdown on Mendip. Upper Knowle. Dundry Hill. Leigh Down. VII. VIII.

880. **A. vulgaris,** *With.*

Native; on banks and in dry pastures, very common. VI. VII.

881. A. alba, *L.*

Native; in marshes and damp pastures everywhere. The var. β *stolonifera* has been gathered on sea-sand at Burnham and Weston-super-Mare, and on mud in the New Cut by Bathurst Wharf. VII.

POLYPOGON, *Desf.*

882. P. monspeliensis, *Desf.*

Alien. Until a few years ago this beautiful grass was connected with the Bristol Flora solely through a communication ("Near Bristol") made by Miss Alice Worsley (afterwards Mrs. Russell) to Mr. Hewett Watson, and published in the *New Botanists' Guide*, 1835. In 1883 it was found growing luxuriantly upon the material dredged from the bed of the Avon and from the basins, which had been placed the year before in a river-side quarry. It has continued there to the present time. Although the circumstances under which it was gathered by Miss Worsley are unknown, the recurrence of this species on the bank of the river goes toward showing that it may formerly have been an inhabitant of the Avon valley.

883. P. littoralis, *Sm.*

Possibly native in St. Philip's Marsh, G., where we have known it many years.

In weighing the claim of this plant to be indigenous in a locality where the original soil is in process of conversion into bricks and pottery, and of replacement by mounds of ashes, rubbish, and refuse of all sorts, it should be borne in mind that, in its primitive condition, St. Philip's Marsh undoubtedly was a fitting station for the species under notice, which in this country grows only near the sea or in marshes washed by tidal rivers. Unhappily, our interest in the question whether *P. littoralis* truly belongs to the aboriginal Bristol Flora is overborne by the certainty that in a little while it will exist only in our herbaria.

GASTRIDIUM, *Pal. de Beauv.*

884. **G. lendigerum**, *Gaud.*

Native; scattered over a very small area on the Gloucestershire bank of the Avon, below Bristol. There is little doubt that all the published records of the occurrence of this grass on St. Vincent's Rocks, Durdham Down, and the bank of Avon, may be referred to the same small tract of limestone upon which it now grows; although its extent is less than it was formerly. Mr. T. B. Flower tells us that he used to find it behind the New Hotwell House, long since removed. That spot is situate on the river-bank, some distance from the present station for the plant. VII. VIII.

HOLCUS, *Linn.*

885. **H. lanatus**, *L.*

Native; in meadows, pastures, hedge-banks, and waste places. Very common and universally distributed. VI. VII.

886. **H. mollis**, *L.*

Native; in woods, hedge-banks, and pastures, very much less common than the preceding species. VI. VII.

AIRA, *Linn.*

887. **A. cæspitosa,** *L.*

Native; in moist woods and damp hedges. Common and abundant in suitable positions.

We have observed the var. *brevifolia* in Leigh Woods. VII.

888. **A. flexuosa,** *L.*

Native; on heaths, banks, and dry open spaces in woods, frequent.

G. Berkeley. Dursley. Conham, and Trooper's Hill; *Herb. Stephens.* Glen Frome. Stapleton. Mangotsfield. Sodbury. Wotton-under-Edge.

S. Lane between Abbot's Leigh and the Tanpits. Hedge-banks between Portbury and Tickenham, and on the high ground near Wraxall. Court Hill and Norton Wood, Clevedon. Blackdown on Mendip. On Lansdown, near Bath. In several places near Wells. VII.

889. **A. caryophyllea,** *L.*

Native; in dry places, rare.

G. Almondsbury. On the pennant at Conham.

S. Abundant on Leigh Down. Brean Down. Near Keynsham. V.

890. **A. præcox,** *L.*

Native; on dry, rocky, or sandy soil, chiefly on the coal measures, frequent.

G. Brandon Hill, still in plenty. On Durdham Down, sparingly. Mangotsfield Common. Conham; *Mr. W. E. Green.* Yate Common.

S. Between Brislington and Keynsham, plentiful. On the Court and Strawberry Hills, and in other spots about Clevedon, apparently confined to the pennant formation; *Mr. D. Fry.* IV. V.

TRISETUM, *Pers.*

891. T. flavescens, *Beauv.*

Native; in meadows and dry places, very common. VII.

AVENA, *Linn.*

892. A. fatua, *L.* *Wild Oat.*

Colonist; on the borders of cultivated fields and among the crops, rather rare.

G. Henbury. Horfield. Fishponds.

S. Near Abbot's Leigh. Portishead. Plentiful in fields on the coast near Woodspring, 1883 and 1885. Occasionally near Wells; *Miss Livett.* Weston-super-Mare. Frequent about Bath; *Fl. Bathon.* VII.

893. A. pratensis, *L.*

Native; on limestone hills and banks, frequent.

G. In plenty on Clifton and Durdham Downs. Kingsweston Down. Stinchcombe Hill. Nibley Knoll.

S. Brean Down. Leigh Down. Common on the Mendip Hills, as at Winterhead, Shipham, and Cheddar. Hilly pastures near Bath. VI. VII.

894. A. pubescens, *L.*

Native; on downs, dry hills, and sometimes in meadows. Much more common than the last, and not so strictly confined to the carboniferous limestone strata, although showing a decided preference for that formation.

G. St. Vincent's Rocks. Clifton and Durdham Downs. Henbury. Sodbury. Wotton-under-Edge and Nibley Knoll. Wyck. Yate.

S. Leigh Down. Bedminster Down. Brean Down.

In pastures at Whitchurch. Pastures between Bedminster and Bourton, and in the railway cutting. Frequent at Clevedon. Keynsham. Wraxall. Cranmore and Milton Hill, Wells. Hilly pastures near Bath. VI.

ARRHENATHERUM, *Pal. de Beauv.*

895. **A. elatius,** *M. & K. Oat-grass.*

Native; in hedges, pastures and cultivated fields. Very common, and generally distributed.

The *var. β nodosum*, with the base of the stem enlarged into a string of bulb-like knobs, is very abundant, especially in cultivated ground. VI. VII.

TRIODIA, *R. Br.*

896. **T. decumbens,** *Beauv.*

Native; on downs, heaths, and dry hills, rare and local.

G. Clifton and Durdham Downs. Yate Common.

S. Leigh Down. Pasture near Brislington. On the Mendips above Draycot. Near Claverton, *Fl. Bathon.* Dulcote Hill, Wells. VI. VII.

KŒLERIA, *Pers.*

897. **K. cristata,** *Pers.*

Native; on downs and dry banks, rather common.

G. Very abundant nearly over the whole of Clifton and Durdham Downs, particularly near the river. St. Vincent's Rocks. Wyck Rocks.

S. Leigh Down. Brean Down. Upper Knowle Clevedon, abundantly. On the hill-side at Weston-super-Mare. Cheddar. Shipham. Winterhead. VI. VII.

MELICA, *Linn.*

898. **M. uniflora,** *Retz.*

Native; in woods and shady places, common.

G. Berkeley. Dursley. Wotton-under-Edge. Tortworth. Cook's Folly Wood. Blaise Castle Wood. Sea Mills.

S. Leigh Wood. St. Ann's Wood, Brislington Bishport. Barrow Gurney. Clevedon. Congresbury. Portbury. Portishead. Weston-in-Gordano. Wells. V. VI.

MOLINIA, *Schrank.*

899. **M. cœrulea,** *Moench.*

Native; on wet heaths and commons, rare and local.

G. Yate Common. Specimens in the Stephens Herbarium show that it formerly grew on Durdham Down, where it is no longer to be found.

S. Abundant on the Mendip Hills, at Blackdown, and about the Mineries. VII. VIII.

POA, *Linn.*

900. **P. annua,** *L.*

Native; very common everywhere. III.—IX.

901. **P. nemoralis,** *L.*

Native; in woods and hedges, frequent.

G. St. Vincent's Rocks. Blaise Castle Wood. Stoke Lane, Stapleton. Copse between Horfield and Filton. Bitton. Westbury-on-Trym.

S. Leigh Woods. Brislington. Flax Bourton. Portishead. Walton-in-Gordano. Wells. VI. VII.

902. **P. trivialis,** *L.*

Native; in meadows, pastures, and waste places, very common and generally distributed. VI.

903. P. pratensis, *L.*

Native; in meadows, pastures, and waste places, very common and generally distributed.

The *var. β subcœrulea* is the common grass of wall tops in the vicinity of Bristol. VI. VII.

904. P. compressa, *L.*

Native; on dry banks and walls, chiefly by the tidal Avon, rather rare.

G. Bank of Avon at intervals from Sea Mills to the Suspension Bridge.

S. Bank of Avon under Leigh Wood. Bedminster. Clevedon.

The wall-top variety of *P. pratensis* is sometimes reported for this species. VII.

GLYCERIA, *R. Br.*

905. G. aquatica, *Sm.*

Native; in ditches, rivers, and ponds. Common and abundant in the marshy lowlands of North Somerset.

G. Bank of Avon above Bristol at Conham. Pilning. Littleton-on-Severn.

S. Bank of Avon at St. Ann's, Brislington. Canal at Radford. Marsh ditches throughout the Cheddar Valley to Brent, Burnham, and Highbridge. VII.

906. G. fluitans, *R. Br.*

Native; in water, very common. VI.—VIII.

SCLEROCHLOA, *Beauv.*

907. S. maritima, *Lindl.*

Native; in salt marshes and on the muddy banks of tidal rivers, and inlets from the Channel. Abundant in the mud on both sides of the Avon.

VI. VII.

908. **S. procumbens,** *Beauv.*

Native; on waste ground near the banks of the tidal Avon, very local.

G. St. Philip's Marsh. New Cut, by the General Hospital. Hotwells. Bank of the river under the Downs, and here and there as far as Shirehampton. Abundant on dredgings deposited in the Black Rock Quarry, 1883 and 1884.

S. Bank of the river at Rownham. VI. VII.

909. **S. distans,** *Bab.*

Native; in damp waste places by tidal water, local.

G. In many places near the Avon, from St. Philip's Marsh to Shirehampton.

S. Similarly by the New Cut, and at Rownham Ferry. Burnham. VI.—VIII.

910. **S. rigida,** *Link.*

Native; on and under old walls, and in other dry places. Very common. VI. VII.

911. **S. loliacea,** *Woods.*

Native; in one or two spots near the Bristol Channel, very rare.

S. Sparingly on the hill-side near the pier at Birnbeck, Weston-super-Mare. Burnham; *Miss Livett.*

BRIZA, *Linn.*

912. **B. media,** *L.* *Quaking Grass.*

Native; in meadows and dry pastures, very common.

VI.

(*B. minor,* L. There is a specimen in the herbarium of the Bristol Naturalists' Society, stated to have been collected on St. Vincent's Rocks by Dr. Dyer. It was doubtless a casual introduction.)

CATABROSA, *Pal de Beauv.*

913. **C. aquatica,** *Beauv.*

Native; in ponds and ditches, frequent.

G. Alveston. Berkeley. Bank of Avon above Bristol. Hallen. Henbury. Filton Meads. Pools about Horfield Common. Edges of ponds near Chipping Sodbury and Yate. Siston.

S. Meadows by Lock's Mills. Ditches at Portbury and towards Portishead. Nailsea. Yatton. Marsh ditches and ponds near Woodborough and in the Cheddar Valley. Pools on Mendip near the Mineries. Peat-moors below Wedmore. Common near Bath; *Fl. Bathon.* VI. VII.

CYNOSURUS, *Linn.*

914. **C. cristatus,** *L.* *Dog's-tail Grass.*

Native; in meadows and pastures. Very common and generally distributed. VII. VIII.

DACTYLIS, *Linn.*

915. **D. glomerata,** *L.* *Cock's-foot Grass.*

Native; in meadows and pastures. Very common and generally distributed. VI. VII.

FESTUCA, *Linn.*

916. **F. uniglumis,** *Sol.*

Native; on sea-sands, very local.

S. Abundant on dunes and among loose sand on the Channel shore above Burnham. VI.

917. **F. sciuroïdes**, *Roth.*

Native; on dry banks and waste sandy places, showing a partiality for the pennant formation. Frequent.

G. On banks about Clifton and Durdham Downs. Dry places on the bank of Avon under the Downs. Brandon Hill. On quays by Cumberland Basin Plentiful on rubble about the pennant quarries in Glen Frome. Troopers' Hill, by Crew's Hole. Mangotsfield. Pucklechurch.

S. Dry places on the bank of Avon below Bristol. On walls and dry rocky banks at Clevedon; *Mr. D. Fry.* Between Brislington and Keynsham, on coal-measures. On the hill at Weston-super-Mare.

VI. VII.

918. **F. Pseudo-Myurus**, *Soyer.*

Native; on dry waste ground, rare.

G. In plenty at the foot of the rocks under Durdham Down. On the mounds of scoriæ, Crew's Hole. On old colliery *débris* near Warmley. Stapleton; *Herb. Stephens.* Near Mount's Hill, Kingswood; *Dr. Hassé.* Sparingly on a rocky bank close to Thornbury station, June, 1884. Wyck Rocks; *Add. Fl. Bathon.*

S. About the quarries on the bank of Avon, under Leigh Wood. Stockwood; *Herb. Stephens.*

VI. VII.

919. **F. ovina**, *L.*

Native; in dry pastures, commons and banks, common.

VI.

920. **F. rubra**, *L.*

Native; on the Downs, banks, and dry pastures, common. VI.

921. F. oraria, *Dum.*

Native. Mr. J. G. Baker has considered some plants gathered on the bank of Avon, in Ashton Fields, S., to be the *F. subulicola* of Leon Dufour and other botanists; *Swete, Fl.* 92. As this Festuca is generally distributed on European sea-coasts, it is highly probable that we shall also find it among the sand dunes between Burnham and Brean.

922. F. gigantea, *Vill.*

Native; in woods and hedges, rather common.

G. Clifton Down. Shady banks of the Frome, near Stapleton. Filton. Henbury. Copse near New Passage. Between Thornbury and Littleton-on-Severn. Tortworth. Wotton-under-Edge.

S. Thickets on Bedminster Down. St. Ann's Wood, Brislington. Between Abbot's Leigh and the Tanpits. Flax Bourton. Clapton. Congresbury. Abundant about Clevedon. Walton-in-Gordano. Wrington. Frequent near Wells. In woods and hedges near Bath, plentiful. VII.

923. F. elatior, *Sm.*

Native; in meadows and by the sides of ditches and streams, especially near the coast. Rather common.

G. Marsh under Ashley Hill. Bank of Avon at Hanham and Conham, and again below Bristol, plentiful. Sodbury. Stapleton. Tortworth. Pilning. The Passages.

S. Bank of Avon near Keynsham, and it grows in very large tufts on the edge of the tideway below Bristol. Ken. Yatton. Wells. Frequent on ditch-banks in the lowlands about Brent, Burnham, and Highbridge. VI. VII.

924. F. pratensis, *Huds.*

Native; in damp rich meadows, by no means common close to the city. We have observed it in Ashton Fields; in meadows above Sea Mills; at Long Ashton; Brislington; and on the slopes of Maes Knoll. At a greater distance it becomes frequent in grass-fields. The *var. β loliacea* has been noticed between Redland and Horfield, and at Henbury, in the northern division of the district. In Somerset we have it recorded from Bedminster, Bishport, Brislington, and Portishead. VI. VII.

BROMUS, *Linn.*

925. B. erectus, *Huds.*

Native; on downs and dry banks, in pastures and by roadsides. Common.

G. Abundant on Durdham Down and the slopes of the Gully. St. Vincent's Rocks. On the railway embankment between Sea Mills and Shirehampton. Rough pastures between Horfield Common and Filton Meads. Henbury. In profusion on the slopes of Nibley Knoll, and on the other hills in the same neighbourhood. Pucklechurch. Westbury-on-Trym.

S. Leigh Down. Bedminster Down. Borders of fields near Abbotsleigh. Very abundant on roadsides between Upper Failand, Wraxall, and Tickenham. At Upper Knowle, and between Knowle and Brislington; flourishing both among the mowing grasses and also upon the old stone walls. Abundant in a hilly field at Keynsham; *Mr. D. Fry.* Near Park Farm, Clevedon; also on the coast towards Portishead. Great Elm. Wells. VI. VII.

Var. β villosus. Combe Hay, near Bath; *Syme, E.B.* Easton, *Miss Livett.*

926. **B. asper,** *Murray.*

Native; in woods, thickets, and damp shady hedges. Rather common and generally distributed. VII.

927. **B. sterilis,** *Linn.*

Native; on old walls, roadsides, and dry, waste places, very common. VI.

928. **B. madritensis,** *L. a. B. diandrus,* Curtis.

Native; on banks, and rocky slopes near the Avon, below Bristol. Only upon the carboniferous limestone; very local, but fairly abundant.

G. On St. Vincent's Rocks, and scattered along the Downs and at the foot of the quarries as far as the Sea Wall.

S. Bank of Avon under Leigh Wood, exactly opposite to the Gloucestershire station. VI. VII.

SERRAFALCUS, *Parlatore.*

929. **S. secalinus,** *Bab.*

Casual; on the quays, in waste places, and as a weed on cultivated ground, rather rare.

G. St. Philip's Marsh. Bank of Avon. Sneyd Park. On dredgings deposited in the Black Rock Quarry.

S. Railway embankment near Hallatrow. Roadside at Woodborough. VI. VII.

930. **S. racemosus,** *Parl.*

Native; in meadows and pastures, common. VI.

931. **S. commutatus,** *Bab.*

Native; in dry pastures and cultivated ground, and by roadsides. Not so common as the last species. VI. VII.

932. **S. mollis,** *Parl.*

Native; in meadows, pastures, and waste places. Very common, and generally distributed. V. VI.

933. **S. arvensis,** *Godr.*

Casual or Colonist; on cultivated ground and waste places, rather rare.

G. Bank of Avon, under the Downs. On the quays, near Prince Street. On old colliery *débris,* near Warmley. Hanham. On dredgings deposited in the Black Rock Quarry, 1883 and 1884.

S. In clover-fields at Whitchurch; *Rev. W. H. Painter.* Cornfield between Bedminster and Bishport. VII.

BRACHYPODIUM, *Pal. de Beauv.*

934. **B. sylvaticum,** *R. & S.*

Native; in woods and hedges. Very common and abundant. VII.

935. **B. pinnatum,** *Beauv.*

Native; on limestone hills, rare.

G. Abundant on Stinchcomb Hill. Gorge near Black Rock; *Herb. Stephens.* In addition to the specimens in the Stephens Herbarium, there is plenty of evidence that this grass formerly grew on rocky slopes above the Avon, towards the Sea Wall. We have searched for it frequently without success.

S. Abundant at the Fir-wood and Strawberry Hill. Clevedon. In plenty on Crook's Peak. At Charlcombe; *Fl. Bathon.* VII.

The plant occurs in large patches of a bright yellowish green colour. This brilliant tint contrasts strongly with the brownish hue of the turf around, and renders the spots conspicuous at a long distance.

TRITICUM, *Linn.*

936. **T. caninum,** *Huds.*

Native; in thickets and woods, rather rare.

G. Sparingly in thickets on Clifton Down. Combe Dingle. Henbury. Bitton. Stapleton. Alveston; *Herb. Powell.*

S. Brislington. King's Wood, near Yatton. Portishead. Woods at Walton-in-Gordano. Wookey Hole and Ebbor Rocks; *Mr. J. G. Baker.* Frequent near Frome; *Dr. Parsons.* VII.

937. **T. repens,** *L.* *Couch Grass.*

Native; in cultivated ground and waste places. Common and generally distributed.

The awned variety, *β barbatum*, has been remarked on ditch-banks at Portbury, at Congresbury, and elsewhere. VI.—VIII.

938. **T. pungens,** *Pers.*

Native; on the banks of tidal rivers, and in muddy waste places near the Channel. Locally common. The forms *littorale* and *pycnanthum* are well represented by the Avon estuary below Bristol. VII. VIII.

939. **T. acutum,** *D. C.* *T. laxum*, Fries.

Native; specimens gathered by Miss Atwood on the banks of the river Avon were authenticated by Mr. Baker. *Swete, Fl.* 94. We cannot find it; but the plant is so generally distributed in maritime districts that we see no reason to doubt that it exists upon our extensive coast-line, although it may no longer grow where Miss Atwood detected it. VII. VIII.

940. T. junceum, *L.*

Native; on the coast sands of North Somerset at Burnham, Brean, and Kewstoke, locally common. VI. VII.

ELYMUS, *Linn.*

941. E. arenarius, *L.*

Native; formerly on the shore of the Bristol Channel near Burnham, and elsewhere; probably extinct.

S. "Burnham, Berrow, and Steart, *J. C. Collins' MSS.*" *New Bot. Guide.* In August, 1880, we found a small quantity in a cove on the coast near Woodspring Priory. The plant grew in a patch of about two square yards on the shingle above high-water mark. At that date the flowering-stems were, many of them, four feet or more high, and bore spikes nearly a foot long. We saw it again in 1881, but on the next visit, in 1884, there was none remaining. Cattle from an adjoining pasture had made their way down the low cliff at the back, and were seemingly answerable for the destruction, by having trampled a path over the spot.

HORDEUM, *Linn.*

942. H. pratense, *Huds.* *Meadow Barley.*

Native; in moist meadows and grass-fields, common. Extremely abundant in the lowland pastures near the Severn and Bristol Channel, forming a very considerable portion of the crop.

G. Alveston. Berkeley. Dursley. Charfield. Littleton-on-Severn. Queen Charlton. Henbury. Pucklechurch, in great abundance. Pilning. The Passages. Shirehampton.

S. Bedminster Meads. Bishport. Long Ashton. Nailsea. Wells. Yatton. Plentiful in maritime pastures towards Brean, Burnham, and Highbridge. VII. VIII.

943. **H. murinum,** *L.*

Native; by roadsides, and in dry, waste places, common. VI. VII.

944. **H. maritimum,** *Withering.*

Native; in salt marshes and in sandy places near tidal water. Rather rare.

G. Bank of Avon, below Bristol, from Sea Mills to Avonmouth. Here and there between Avonmouth and New Passage.

S. Coast sands above Burnham. VI.

LEPTURUS, *R. Br.*

945. **L. filiformis,** *Trin.*

Native; in maritime pastures and on ditch-banks in salt marshes; sometimes fringing the edges of the muddy estuaries. Common in such situations.

G. New Cut, near the Hospital. Plentiful on the verge of the mud-banks near Rownham Ferry. Bank of Avon below the Suspension Bridge at intervals down to the mouth of the river. Shirehampton Marshes.

S. Bank of Avon at Rownham. In fair quantity on the mud-flats outside the sea-bank below Clevedon; *Mr. D. Fry.* Sands west of Weston-super-Mare.

Marshy sands near Berrow. Ditch-banks and salt marshes between Burnham and Highbridge, on the banks of the Brue. VII. VIII.

LOLIUM, *Linn.*

946. **L. perenne,** *L.* *Rye Grass.*

Native; in meadows and pastures, and by roadsides; very common and generally distributed.

A very curious form, assuming a stoloniferous habit. grows in sea-sand at Kewstoke, and also at Burnham. VI.

947. **L. italicum,** *A. Braun.*

Alien. Half naturalised. Introduced by cultivation, Common among mowing-grass, by roadsides, and in waste places. VI.

948. **L. temulentum,** *L.*

Casual or Colonist; in cultivated fields and waste places, rare.

G. Prince Street, Bristol. Roadside at Sneyd Park. Cornfields, Alveston; *Herb. Powell.*

S. In a barley-field near Nailsea, 1880. Roadside at Ken, 1881. In several cornfields near Bath; *Mr. T. B. Flower.* VII. VIII.

PHANEROGAMIA.

Class 3. *GYMNOSPERMÆ.*

CONIFERÆ.

TAXUS, *Linn.*

949. **T. baccata,** *L.* *Yew.*

Native; in nearly all old woods upon the carboniferous limestone. Locally common.

It is very abundant in Leigh Woods, and about the rocky combes and hills at Brockley, Cleeve, and Congresbury. There are some especially fine trees in the churchyard at West Harptree, and in the villages of Churchill and Compton Martin; but perhaps the finest and most perfect yew grows in Winscombe churchyard, where its sheltered position has preserved it from the shattering storms of centuries. A larger and more ancient tree is enclosed in the vicarage grounds. These trees may have been planted when Winscombe manor was a possession of Glastonbury Abbey. III. IV.

JUNIPERUS, *Linn.*

950. **J. communis,** *L.* *Juniper.*

Native; abundant on some hill-sides east of Bath; on the extreme border of our district. Dr. St. Brody, in his *Flora of Weston*, gives a locality near Uphill, which has not been confirmed. V.

CRYPTOGAMIA.

EQUISETACEÆ.

EQUISETUM, *Linn.*

951. **E. arvense,** *L.*

Native; in damp fields and on banks and roadsides, very common. IV.

952. **E. maximum,** *Lam.*

Native; in wet places in woods, ditches, and hedges, frequent.

G. Ashley Hill and Baptist Mills. Hedge-banks between Eastfield and Filton. Queen Charlton. Westbury-on-Trym.

S. Wet hollow in Leigh Wood near Rownham Ferry. Lane on Maes Knoll. Chew Magna. Clevedon. Kewstoke. Woodspring. Woodborough. Yatton.

We have two specimens of the fertile stem, in which the terminal cone has its upper half divided, one into five and the other into eight, erect branches. IV.

953. **E. limosum,** *L.*

Native; in stagnant water, ditches, and swamps, frequent.

G. Between Thornbury and Littleton-on-Severn. Horfield.

S. Pond near the lodge in Leigh Woods; *Herb. Stephens.* Bedminster Meads. Abundant below the reservoirs of the Bristol Water Company under Dundry Hill, 1881. Walton Drove, Clevedon. Very plentiful in marsh ditches about Draycot and elsewhere in the Cheddar Valley. Yatton. In the canal by Bath; *Add. Fl. Bathon.* VI. VII.

954. **E. palustre,** *L.*

Native; in marshes, swamps, and ditch-banks, frequent.

G. Marsh at the Boiling Well. Filton Meads.

S. By the Abbot's Pond, near Abbot's Leigh. Bedminster Meads. Clevedon. Winscombe. Yatton. Abundant on ditch-banks between Brean and Berrow. Frequent in boggy places; *Fl. Bathon.* VI. VII.

FILICES.

POLYPODIUM, *Linn.*

955. **P. vulgare,** *L.* *Common Polypody.*

Native; on shady banks, walls, and old trees, very common. Pinnæ occasionally bifid at the end, sometimes serrate or even (*P. cambricum*, L.) doubly pinnatifid. VIII.—X.

956. **P. Phegopteris,** *L.* *Beech Fern.*

Native; in a damp, mossy dell near Wells, S., where we saw a patch extending some yards, in 1883 and 1884. VII.—IX.

957. **P. Dryopteris,** *L.* *Oak Fern.*

S. In Leigh Wood, rare. *Shiercliff's Guide*, 1789. Leigh Wood, sparingly, 1839. *Mr. T. B. Flower.*

In conversation Mr. Flower has explained to us that this fern formerly grew with others in a damp, boggy hollow near Rownham. We know the place very well; but the ferns are not there now.

958. **P. Robertianum**, *Hoffm.* *Limestone Polypody.*

Native; on limestone, rare and local.

In Leigh Wood, rare. *Shiercliff's Guide*, 1789. We understand that before the construction of the Suspension Bridge and its approaches, the high ground on the Leigh side, above Nightingale Valley, was covered with heath, sand, and loose stones; and that *P. Robertianum* grew among the latter. Plentiful at Cheddar. Brockley. Burrington. Cleeve. Ebbor Gorge; *Miss Livett.* V. VIII.

LASTRÆA, *Presl.*

959. **L. Thelypteris**, *Presl.* *Marsh Fern.*

Native; in wet peat bogs, very local.

S. Once seen in a boggy spot between Portishead and Clevedon; *Mr. R. V. Sherring.* Abundant on the peat-moors at the southern limit of the district. VII. VIII.

960. **L. Oreopteris**, *Presl.* *Sweet Mountain Fern.*

Native; in woods and about heaths and commons, rather rare.

G. Conham; *Herb. Stephens.* Henbury; *Herb. Powell.*

S. Sparingly in Leigh Wood; *Mr. S. Rootsey; Dr. Thwaites;* and *Mr. T. B. Flower.* Ashton Manor Woods; *Miss Atwood; Swete, Fl.* 96. Portbury. Two or three plants in Norton's Wood, by Clevedon; *Mr. R. V. Sherring.* A great many in a small combe between Cleeve and Brockley. In some of the combes of Blackdown. On Mendip near Cranmore Tower, where the old red sandstone occurs; *Dr. H. F. Parsons.* VII.

961. L. Filix-mas, *Presl.* *Male Fern.*

Native; in woods and on hedge-banks, common.

VI. VII.

962. L. spinulosa, *Presl.*

Native; in wet thickets and bogs, rare.

S. Between the hummocks in boggy ground on Mendip, near the Mineries. Leigh Wood, 1881; *Rev. W. H. Painter.* Peat-moors in the extreme south. VIII. IX.

963. L. dilatata, *Presl.*

Native; in woods and thickets, rather common.

VIII. IX.

POLYSTICHUM, *Roth.*

964. P. aculeatum, *Roth.*

Native; on hedge-banks and in woods. Frequent, but no longer to be found in some of the recorded stations.

G. Blaise Castle Woods; *Herb. Powell.* Hanham and Bitton; *Mr. T. B. Flower.* Stapleton Wood; and hedges near the Zoological Gardens, Clifton; *Swete, Fl.* 96.

S. Leigh Woods; St. Anne's Wood; Kelston and Claverton; *Mr. T. B. Flower.* Dundry Hill; *Herb. Stephens.* Hallatrow. Portishead. Damp hedge-banks close to the village of Compton Martin. Lanes near Great Elm.

Var. β *Aspidium lobatum*, Sw.

G. Blaise Castle Woods; *Herb. Powell.* Shirehampton; *Mr. T. B. Flower.*

S. Sparingly in Leigh Woods; at Bourton and on Dundry Down; *Dr. Thwaites.* In several places near Bath; *Mr. T. B. Flower.* Clevedon; *Mr. W. E. Green.* Upper end of Cheddar Gorge.

VII. VIII.

The form or variety *lobatum* is unsatisfactory, inasmuch as it graduates imperceptibly into the type, and *vice versâ.* Our account is substantially correct; all doubtful plants being referred to *aculeatum.*

965. **P. angulare,** *Newman.*

Native; on very shady hedge-banks and in woods. Not at all common near Bristol.

G. Dursley. Frenchay. Tortworth. Wotton-under-Edge. Stapleton Woods; *Swete, Fl.* 96.

S. Leigh Wood. Wood by the river Avon between Pill and Ham Green. Hedges in Upper Failand. Maes Knoll. Banwell. Churchill. Clevedon. Clapton. Great Elm. Beechen Cliff, and wood on Lansdown; *Mr. T. B. Flower.* VII. VIII.

CYSTOPTERIS, *Bernh.*

966. **C. fragilis,** *Bernh. Bladder Fern.*

Native; on rocks and walls, showing a great partiality for the carboniferous limestone.

G. A few plants on a wall at North Stoke, 1878.

S. Nightingale Valley, Leigh Woods; *Herb. Stephens.* Dr. Stephens' specimens are very fine. Recorded from the same locality by Mr. Leo. H. Grindon, and by Dr. Thwaites, about 1840. The latter botanist sent examples from Nightingale Valley to the London Botanical Society, a fact which leads us to suppose that the fern was then plentiful. At the present time we much doubt if a single plant exists in Leigh Woods. On Dundry Hill, still plentiful. Under fir trees near Brockley, many very large plants. Burrington Combe. Cheddar Cliffs, on both sides of the gorge. In crannies of the water-worn rocks on Mendip above Draycot. Abundant on damp hedge-banks under Dolbery Camp. Of very fine growth in East Harptree Combe. On walls at Chewton Mendip; Gurney Slade; Litton and Stone Easton. Ebbor. Emborrow. Dulcote Hill, near Wells. On walls near the Mineries on Mendip. Stanton Drew. Yatton. VII. VIII.

ATHYRIUM, *Roth.*

967. **A. Filix-fœmina,** *Roth.* *Lady Fern.*

Native; in damp, shady places, frequent.

G. Almondsbury. Pucklechurch. Tortworth. Wood near Stapleton.

S. Several localities in Leigh Woods, and between Abbot's Leigh and Failand. Axbridge. Bourton Combe. Brockley Combe. Norton Wood, Clevedon. Wood between Temple Cloud and Clutton; *Mr. D. Fry.* Sidcot. Frequent near Wells. In several woods near Bath. VI. VII.

ASPLENIUM, *Linn.*

968. **A. lanceolatum,** *Huds.*

Native; on sandstone rocks, very rare.

G. Frome Glen, Stapleton; *Herb. Stephens.* Oldbury Court Woods, and lanes about Stapleton; *Swete, Fl.* 97.

These records relate to one and the same locality, where the plant was discovered by Mr. J. W.

Ewing, of Norwich, who sojourned in Bristol about 1830. See note by Dr. Thwaites in the *Phytologist*, I. 75. Swete (*Fl.* 97) writes that at his date its area was "not more than half a mile," implying that it occurred over a considerable space, which is to some extent confirmed by Dr. Thwaites, *loc. cit.* Undoubtedly the fern has shared the fate allotted by collectors to all good things; for the late Mr. W. W. Stoddart spoke of it ten years ago as being only obtainable with the aid of a quarryman and a rope, and other information is to the same effect.

969. A. Adiantum-nigrum, *L.* *Black Spleenwort.*

Native; on walls and rocks, and occasionally on hedge-banks, frequent.

G. Henbury Combe. Almondsbury. Aust. Tortworth. Wickwar.

S. Abbot's Leigh. Failand. Clevedon. Chew Magna. On rocks at Brean Down. Cheddar Cliffs. South Stoke. Sandford. Shipham. Rarely near Wells. Walton-in-Gordano. Weston-in-Gordano. Near Yatton. VI.—IX.

970. A. Trichomanes, *L.* *Common Spleenwort.*

Native; on walls and rocks, frequent.

G. Frampton. Kingswood. Stoke Gifford. Westbury-on-Trym.

S. Rocks in Leigh Woods. Old walls at Long Ashton. Clapton. Tickenham. Walton-in-Gordano. Shipham. Stanton Drew. On Mendip above Axbridge, Draycot, and Cheddar. Old mine shaft on Dolbery Camp. In all the parishes at the back of the Mendips. Wells. Frequent about Bath. V.—X.

971. A. marinum, *L.*

Native; in the crevices of rocks on the coast of North Somerset, very rare.

S. Between Portishead and Clevedon; *Herb. Stephens.* It grows on rocks at Walton-in-Gordano, and between there and Portishead. Sparingly on rocks at Brean Down; *Mr. T. F. Perkins*, 1881.

972. A. Ruta-muraria, *L.* *Wall Rue.*

Native; on rocks and old walls, very common. It may often be seen in the older parts of Clifton; as underneath Royal York Crescent, and on Richmond Terrace. V.—IX

SCOLOPENDRIUM, *Sm.*

973. S. vulgare, *Sym.* *Hart's-tongue.*

Native; in damp, shady places, common. We have seen plants bearing fronds bifid, crisped, or contorted in various ways. VII. VIII

CETERACH, *Willd.*

974. C. officinarum, *Willd.* *Rustyback.*

Native; on old walls, rocks, and banks, common. Very abundant about Bristol, and one of the prettiest ornaments of our limestone walls.

G. In plenty at Almondsbury. Rocks at Penpole Point. Frenchay. Thornbury. Westbury-on-Trym.

S. Limestone rocks in Leigh Woods. Old walls at Long Ashton. Flax Bourton. Banwell. Congresbury. Churchill. Clevedon. On Mendip at Cheddar and Draycot. Downside. Compton Martin. Tickenham. Stanton Drew. Stone Easton. Walls about Pill and Ham Green. Rocks at Brean Down. Wells. Winscombe. Yatton. Abundant in all the parishes at the back of the Mendips. Frequent on walls near Bath. IV.—IX.

BLECHNUM, *Linn.*

975. **B. boreale**, *Sw.* *Hard Fern.*

Native; in woods and on commons, rather rare. Nearly absent from the neighbourhood of Bristol, and, in common with all other ferns, much reduced in quantity by the ravages of itinerant street hawkers, who bring the roots into Bristol for sale.

G. Wyck. Yate Common.

S. Still plentiful in some of the preserved portions of Leigh Wood. Formerly in St. Anne's Wood, Brislington. Blackdown. Sparingly in Cleeve Combe. Norton Wood, Clevedon. Sparingly near Wells. Near Yatton. VII. VIII.

PTERIS, *Linn.*

976. **P. aquilina**, *L.* *Brakes or Bracken.*

Native; in woods, and on heaths and commons. Very common, except in the alluvial lowlands.

VII. VIII.

(*Adiantum Capillus-Veneris*, L. We have come across several accounts of the capture of maiden-hair ferns in various localities in the vicinity of Bristol and we give them for what they are worth.

"Under a bridge at Compton Dando, S., where it has been known some years."

"At the mouth of an old well near Clevedon"; quoted in *Cyb. Brit.*, vol. iii.

"Three plants, growing in the air-shaft of a stone quarry some thirty feet below the ground, at Combe Down, near Bath," 1853. *E. J. Lowe; Phytol.* iv. 1100.

"In the year 1851 I found a plant or two of it on moist rocks in the neighbourhood of Cheddar, in an out-of-the-way situation, and left the roots uninjured." *W. H. Hawker; Phytol.* v. 82.)

(*Hymenophyllum tunbridgense*, Sm. In a shady lane near Shepton Mallet; *Sole, MS.* 1782. Not confirmed.)

OSMUNDA, *Linn.*

977. **O. regalis**, *L.* *Royal or Flowering Fern.*

Native; in peat bogs and swamps, very rare.

S. An old publication (*West of Engl. Journ. of Science and Literature*) states that it formerly grew in Leigh Woods. Formerly in a wet copse on Walton Moor; now extinct. On the Burtle turf moor, north of the railway, July, 1881. At one time it extended north as far as Wedmore; but is now chiefly confined to the southern turf moors outside the district. VII. VIII.

BOTRYCHIUM, *Sw.*

978. **B. lunaria**, *Sw.* *Moon Wort.*

Native; on downs and hilly pastures, rare.

G. Kingsweston Hill; *Miss Powell, Swete, Fl.* 98. Penpole Point; *Mr. W. W. Stoddart.*

S. Clevedon; *Mr. E. Green.* By Walton Castle, Clevedon; *Mr. T. B. Flower.* Callow Hill, near Sidcot; *Herb. Stephens.* On the hills about Winscombe. One plant in a field on Tining's Farm, near Cheddar, with *Vicia Orobus;* June 27, 1883. Between Claverton and Bath; *Add. Fl. Bathon.* Pen Hill, near Wells. V.—VII.

OPHIOGLOSSUM, *Linn.*

979. **O. vulgatum,** *L. Adder's Tongue.*

Native; in damp pastures, and on grassy roadsides, common.

G. In fields under Ashley Hill, and near the Duchess Ponds, Stapleton. Filton Meads. Haw Wood, and Blaise Castle Wood; *Herb. Powell.* Frome Glen; *Swete, Fl.*, 98. Very abundant in meadows between Henbury and Compton Greenfield, 1886. Stoke Gifford. Thornbury.

S. Roman encampment, Leigh Wood. Grassy bank by the side of a road near the Abbot's Pond. Brislington. Failand. Abundant in fields between the Bridgewater road and Dundry Hill. On the hill at Kewstoke. Fields at Breach Hill, near Chew Stoke; and at Compton Martin. Orchards at Winscombe. Pastures near Wells. Yatton. Frequent about Bath; *Fl. Bathon.* V. VI

LYCOPODIACEÆ.

LYCOPODIUM, *Linn.*

980. **L. clavatum**, *L.* *Common Club Moss.*

Native. Among long grass in open ground, on a hill near Clevedon. Stated in the *Phytologist* to have been abundant towards the beginning of the century. In fair quantity, 1885.

981. **L. Selago**, *L.* *Fir Club Moss.*

Native; with the last species, very sparingly. Discovered by Mr. Mason in 1884. The plant is fairly common in the west of the county.

CHARACEÆ.

CHARA, *Linn.*

982. **C. flexilis**, *L.*

S. In the canal at Bath; *Fl. Bathon.*

983. **C. fœtida**, *A. Br.*

G. Syston; *Dr. Hassé.* Eastfield.

984. **C. hispida**, *L.*

S. Walton-in-Gordano; *Kew Herbarium.* In th canal at Bath; *Fl. Bathon.*

985. **C. aspera**, *W.*

S. Ditches near Portbury. Clevedon. Yatton.

986. **C. vulgaris**, *L.* var. *longibracteata.*

S. Pool between Bedminster and Whitchurch, 1882.

The Characeæ have not received a fair measure of attention.

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Bristol Building Stones.

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"COMMENCING with what are commonly called building materials," writes Prof. Ansted in his *Cantor Lectures* (1865), "we may regard them as of three classes—granites, sandstones, and limestones. Practically there is another division of the whole number into two classes; namely, those which can only be worked by the pick or by wedges, and those which can be worked by the mallet and chisel. The latter are called *freestones*, and include marbles, all limestones, and stratified sandstones. The former include granites, quartz rocks, and indurated schists, conglomerates or pudding-stones, in which quartz predominates, and some agglomerated sandstones that cannot be fitly called pudding-stones. Of these some are rarely used on a large scale, except in the immediate vicinity of the rocks whence they are derived, and thus, practically, the number of those which we have to consider is reduced within reasonable limits."

In such a course of lectures, Prof. Ansted could afford to neglect such stones as are "rarely used on a large scale, except in the immediate vicinity of the rocks whence they are derived." But before a local Society it will not, I hope, be deemed unfitting to read a paper on local building stones. I shall however make my remarks so far general as to include such stones as are used in Bristol, though they be brought from a distance.

It is only right that, at the outset, I should acknowledge my indebtedness (1) to my former pupil, Mr. F. W. Hardwick, who aided me in my experiments and in the determination of the specific gravities and absorptive capacities * of our local stones; and (2) to Messrs. W. Brock, John Barstow, and others, for much valuable information most courteously given.

Of the three classes of building materials mentioned by Prof. Ansted, one, the granite group, including the elvans, porphyries, and traps, has not been extensively used in Bristol. The widening of Bristol Bridge was executed with Cornish granite. Lundy granite was, I am told, used for the harbour works at Cumberland Basin. For such a purpose no stone could be better adapted than granite, since it absorbs but little water. A cubic foot of the rock in the ordinary dry condition (in which it already contains about one pint of water, which can only be liberated by artificial heat) is capable of absorbing scarcely three-tenths of a pint more on being placed in water. In a frost-bitten land, this small absorptive power is essential in a building material for such purposes. Granite is also a rock which offers great resistance to crushing stress, though different classes vary considerably in this respect. A cubic inch of Cornish granite crushes under a load of 2·81 tons, while Mount Sorrel

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* On this subject see also Mr. E. Wethered's Paper on the Porosity and Density of Rocks with regard to Water Supply (*Proc. Brist. Nat. Soc.*).

syenitic granite does not yield till the load reaches 5·74 tons.

In granite districts, where this rock is locally used for ecclesiastical purposes, the work is heavy, the material being too stubborn to admit of grace or lightness of finish.

Ornamental pillars of polished Scotch and Cornish granite are used in the Promenade and other parts of Bristol. They are also conspicuous in the interior of the Church of St. James-in-the-Barton, and in that of Norton Malreward, Somersetshire. The introduction of polished granite pillars in a building containing Norman work is a somewhat torturing piece of "barbaric splendour."

I am not aware that any trap rock is used in construction in our own neighbourhood. Indeed the use of such rock is exceptional. A brown trap has however I believe been used with considerable effect in the roof of Exeter Cathedral.

Passing from the granites to the sandstones, there are three local rocks of this class used in the neighbourhood of Bristol,—Millstone-grit (locally "fire-stone"), Pennant, and Old Red Sandstone.

Millstone-grit, or fire-stone, is an exceedingly hard, tough rock, composed of grains of sand cemented by a silicious or felspathic cement. The specific gravity is 2·62. A cubic foot of the rock weighs about 161 lbs. Subjected to crushing stress in the testing machine in the University College, Bristol, a cubic inch of a close-grained variety of this rock from Long Ashton quarry began to split at 10·17 tons, and broke down at 13 tons. A two-inch cube of a somewhat less compact rock from the same quarry began to split at 11·17 tons, and broke down at 19·39 tons. The former gives the highest resistance of our local stones. (See Appendix.) The more absorbent varieties of this stone as used in construction seldom take up more than one pint to the cubic foot. Less absorbent varieties take up scarcely one-third of this amount. The percentage absorption by volume is from 1·86 to ·7.

The extreme hardness of the rock renders it difficult to work, and therefore expensive in construction; nor can blocks of any size be obtained. It is however an exceedingly durable stone, and its rich red colour, and somewhat rough facing, is not unpleasing to the eye. Queen Elizabeth's Hospital, the Grammar School, and the Deaf and Dumb Institute are constructed of this stone, as are also the lodges of the Ashton Park Estate. For the latter the rock was obtained from Long Ashton; for the Grammar School, from Brandon Hill. The stone was, Mr. Brock informs me, formerly used for pitcher paving in the streets. The Brandon Hill quarries are no longer worked. Boundary walls formed of this stone are far superior to those in which Mountain Limestone is used.

Pennant Sandstone would seem to be the favourite building stone of modern Clifton, as the Bath stone and Dundry stone were of older Bristol and Clifton. It is obtained from extensive quarries at Hanham, Fishponds, and Stapleton, there being also quarries at Winterbourne, Nailsea, and Clevedon.

It is a coarser grained rock than the Millstone grit used in construction, and a denser stone, though much less hard and durable. The colour is greenish gray, bluish gray, or red, according to locality and conditions of occurrence. The specific gravity is 2·67. The cubic foot of dry rock weighs about 163½ pounds. Subjected to crushing stress in the University College testing machine, it showed no sign of yielding till it suddenly broke down with a load of from 20 to 25 tons on the two inch cube. In this respect Pennant compares favourably with the strongest rocks.

It absorbs from one and a half to three and a quarter pints to the cubic foot. The percentage of absorption by volume is from 6·5 to 2·75. The rock is distinctly bedded, and should be laid down with the bedded layers horizontal, otherwise there is a tendency to flake off under the action of frost and weather, and to produce bad work.

"The great disadvantage of the stone is the disability to stand masons' tools, the face in time peeling off. This can be seen at the portion of the Bridewell now standing, and the plinth of the Council House." Few of us can have failed to notice this fact, to which Mr. Brock alludes in some valuable notes with which he was good enough to furnish me. I have been informed that the use of wooden mallets checks or lessens this vice. "Within the last few years," however, Mr. Brock informs me, "some of the quarry masters have sawn the solid block in the quarry into paving and steps which will not peel or perish on the surface." An example of such sawn blocks may be seen in the flight of steps to the north porch of St. Mary Redcliffe Church, recently renewed by Messrs Brock & Bruce.

For house-building this is now generally regarded as the best stone. It may be seen in the incomplete buildings of the University College, in the Bristol General Hospital, and in several of the newer churches.

The oldest structure in which Pennant has been used in Bristol is the tower of St. Peter's Church, of Norman age. How badly the stone has there weathered is made obvious by the presence of blocks of millstone grit, which stand out, from their greater powers of resistance, several inches beyond the weakened surface of the Pennant. Even granting that little care was taken in selecting the stone for this building, the state of St. Peter's tower leads one to question the advisability of its use in ecclesiastical structures and other buildings intended to outlive the shock of ages. I have made experiments on pieces of this stone such as are in ordinary use for building purposes, and I find that repeated freezing and thawing causes a considerable loss from superficial disintegration.

For paving it is used throughout Bristol and Clifton, and in this respect will, in Mr. Brock's opinion, compare favourably with that of any other city or town in England.

Old Red Sandstone is not much used in the neighbourhood of Bristol. Blocks have been built here and there into the old Cathedral walls, and illustrate well how readily this material, in its more sandy and friable varieties, succumbs to the action of the weather. Old Red Sandstone from a quarry on the Somersetshire side of the Avon has been used for the buttresses of the Suspension Bridge. The Church of Abbots Leigh is built of soft and sandy Old Red Sandstone, which has flaked off badly in parts. In the Church at Westbury, the lower part of the walls on the south side (13th century) are Old Red, which has stood well. In St. Mary's, Portbury, sandy, conglomeratic, and gravelly Old Red has been used, the conglomeratic stones with milky white quartz weathering well. Similar stone has been used in the old tower of St. George's, Easton, while in the recent restoration Old Red (from Markham, I am told), has been used. The new Church, at Failand, has been built of Old Red, quarried on the spot. The stone is hard, and mostly conglomeratic, with milky white quartz, and is of admirable quality. A few sandy stones should have been rejected. Across the Channel, in Tintern Abbey, we have a notable example of this rock in construction. The stone was obtained from the "Barbadoes Quarry" in the vicinity, and though parts have perished badly, in other parts the stone is in good preservation. In Chepstow Castle, built partly of this stone, and partly of Carboniferous Limestone, the sandstone compares very unfavourably with the limestone. A similar sandstone, but of Permian age, may be seen in the ruined Abbeys of Calder and Furness in the North of England.

The rock, as generally used in construction, is fairly close-grained, but friable. The specific gravity is 2·6. The dry stone weighs about 147 pounds to the cubic foot. Subjected to crushing stress, two-inch cubes of a firm, compact stone from Stoke showed signs of yielding to a load of about 7½ tons, and broke down with a load of from 9½ to 12 tons. If I may trust the few experiments I have been able to make, the rock gives 1½ ton (or 16½ per cent.) greater resistance when the lines of bedding are laid horizontally than when they are placed vertically. The absorptive power varies very considerably, the more porous varieties taking up as much as eight pints to the cubic foot; a very compact variety not taking up more than two pints. The percentage absorption by volume is 4·4 to 17·5.

I have made some experiments on compact cubes of this rock with the object of ascertaining the effects of repeated freezing and thawing. The cubes were soaked in water, and placed in tin boxes in a freezing mixture of snow and salt, and were allowed to remain there for from three to four hours. On removing them their surface was found to be frosted over; and, examined under a lens, minute ice-columns were visible, pressed out, I presume, by the expansion of the ice. On allowing the block to thaw and dry, fine sand-dust, loosened by the action of the frost, could be brushed off with a camel's hair pencil. The loss of weight in the case of one two-inch cube was, after twelve alternate freezings and thawings, ·89 gram. I have no doubt that in stones of looser texture the loss would be much greater. Subjected to crushing stress these cubes did not appear to be weakened by the process. One of them gave, indeed, a resistance above the average.

Jurassic sandstone from Yorkshire has, Mr. Brock informs me, been occasionally used in Bristol, the largest job being Bristol Bridge, the stone for which came from Whitby—the Aislaby quarries, I presume. This is the rock that was used for building Whitby Abbey. It is of a light-brown colour and moderately fine grain, the cubic foot weighing $126\frac{3}{4}$ pounds.

Of rocks which fall within the class of limestones, Oolites (Bath, Dundry, and Doulting), Mountain Limestone, and Blue Lias are in use.

The building stones from the Lias are poor in quality, and are seldom used for aught but common walling. The stone is quarried at Horfield, Keynsham, Willsbridge, Saltford, Knowle, Bedminster Down, and Barrow, but is not now used to any great extent. The best beds are at Saltford, where the stone is harder and bluer than at other quarries. The side walls of the old Great Western Railway Station at Bristol are built with this stone. The church at Whitchurch is Lias, this material being frequently used locally. The railway station at Keynsham is built with Keynsham stone, which is also banded in the church tower in that village, where it well exemplifies its tendency to decay. In the church at Westbury there are Lias and local Dolomitic Conglomerate courses, both of which have weathered back deeper than the freestone dressings. "Where the late Bristol and Exeter Railway, now Great Western Railway, passes under the Bath road, there are some piers built with Lias which, though well selected and well laid, are rapidly decaying" (W. Brock). In fact, it matters not where you go, wherever Lias has been used, there will you find the abundant signs of rapid decay.

Mountain Limestone, which is extensively quarried in the immediate neighbourhood, at South Mead, near Westbury-on-Trym, and at Wick, is chiefly used for rough walling. It is an expensive stone to work, being full of joints, which perhaps break when the mason has almost finished the working. When used it is found to be a very durable stone, and good examples may be seen in the boundary wall of the old city Gaol, and the front of the Weighbridge House near the Totterdown Lock. Stones of Mountain Limestone have been built in to the older parts of Westbury Church; and at the west end of the south aisle an old and narrow doorway has been filled in with this stone. It is occasionally used in houses, as in those in Leigh Woods. Mr. Bastow informs me that where this material is used, a $4\frac{1}{2}$-inch brick lining, with a space between this and the limestone, should always be used. The stone has stood well in Chepstow Castle, though it was employed, I believe, only in the later work.

The stone varies from a coarse-grained, shelly, crinoidal, or coralline rock, with a markedly crystalline fracture, to a close-grained oolite of a very uniform texture. The latter is better in construction. Its specific gravity is 2·7, the cubic foot of dry rock weighing nearly 168 pounds. Subjected to crushing stress, a prism two inches square and three inches high, cut from Black Rock Limestone (containing ·0126 per cent. carbon, and 3·3 per cent. silica), began to crack at 19 tons, and broke down under a load of 19·46 tons. A coarse, shelly limestone, containing Spirifers, from near the base of the series showed signs of yielding at from seven to eight tons on the two-inch cube, and broke down when the load reached about twelve tons. The absorptive power is remarkably low, being often less than one-tenth, and rarely more than four-tenths, of a pint to the cubic foot, or from ·2 to ·4 per cent. by volume.

Most of the older buildings in Bristol and Clifton are built of Jurassic Oolite, the even facing of which contrasts markedly with the rough facing of the Pennant now so largely in use. But even in houses where Pennant is now employed, Oolite is employed for dressing.

The Oolite in use is of three kinds—Dundry, Doulting, and Bath. The two former are, geologically speaking, from the Inferior Oolite; the last from the Great Oolite.

Dundry stone, from the outlier of that name, is now but little quarried. It is however being used for the restoration of the Norman Archway House, College Green. It was used throughout in the construction of St. Mary, Redcliffe, Church, where it has stood well on the whole, though in parts it shows signs of exfoliation. Parts of the north porch, and of the south-east wall, and buttresses of the chancel, have been encased with Caen stone, which is perishing miserably. One would have thought that the example of Henry VII. Chapel in Westminster Abbey would have been a deterrent to the use of this material in an atmosphere not always smokeless.

I gather from a note in Mr. John Evans' *Chronological History of Bristol* (1824) that St. Augustine's Abbey (now part of the cathedral) was built of Dundry stone, the abbots holding a lease of the quarries at Dundry; and that the churches of the twelfth century are all of this stone. Certainly the old cathedral walls, and the lower part of St. Stephen's tower, do not seem to have stood well. The upper part of St. Stephen's tower (about 1460) seems to have suffered less in proportion. I do not know of what stone it is built, but in all probability it is Dundry.

Dundry stone is heavier than Bath stone, standing indeed midway between this light freestone and the denser Portland Oolite. Its specific gravity is 2·45, a cubic foot of the dry rock weighing about 126 pounds. Subjected to crushing stress, the cubic inch yields without previous fracture to a load of about 1·2 tons, or 3·6 tons to the two-inch cube. The absorptive power is about eleven pints to the cubic foot, or 18·8 per cent. by volume.

I have made some experiments with cubes and prisms of this rock to test the effect of alternate freezing and thawing. The result was in each case loss of weight due to the dislodgment of particles from the surface. The loss was however less uniform than in the case of Old Red Sandstone, the surface becoming markedly pitted.

How marked an effect frost has upon such soft, absorbent oolitic rock may be seen after any winter in Bristol, where the coping stones are but too apt to crumble to powder. The pitted surface may be also seen in a window frame, where, after a few years, the difference between the inner and outer aspect becomes sufficiently well marked. The Dundry stone pits deeper and more irregularly than the Bath stone, while in the Doulting stone the shelly structure is brought out by the process. Where a vein of spar passes through such a rock, the differential effect is readily observable. The general surface being lowered by the removal of grains, the vein of spar stands out in relief. See, for example, the coping stones for the railings round Victoria Rooms, Clifton. In many places in Clifton the numbers or names of houses on the pillars having been protected by a coat of paint now stand out in relief. The action is partly mechanical by the removal of grains, partly chemical through the action of dissolved carbonic anhydride. If we examine the exterior of old Bristol Cathedral, however, we shall see that the blocks of Old Red Sandstone have yielded to the disintegrating action of frost and weather (purely mechanical in this case), far more than the surrounding

Oolite or the yellowish Triassic stones. Mr. Ansted tells us that "the influence of frost on a stone is in proportion to the water it takes up, and determines its durability." This statement needs qualification, for the durability is largely dependent on the readiness with which the particles of the rock are dislodged by the superficial action of frost, which, in my experiments, is greater in Old Red Sandstone than in Dundry stone, though the absorbent power of the Dundry stone is the greater.

The rate at which a stone disintegrates from the action of frost is determined very largely by the aspect of the exposure. Stones in those parts of a building which have a southerly or south-westerly aspect are apt to disintegrate far more rapidly than those with a northerly or north-easterly aspect. This is in the main due to the fact that during the winter south and south-west walls receive the sunshine of the day after the frost of night, whereas north and north-east walls may remain frozen for weeks at a time. It is not either the long continuance or the intensity of frost that disintegrates the rock; it is the *alternation of frost and thaw.*

In my experiments on Dundry stone I did not find that the alternate freezing and thawing appreciably altered the resistance to crushing stress. Nor did (1) alternate heating and cooling, or (2) alternate soaking and drying produce any weakening effect. All such actions have merely a superficial effect. Disintegration is a matter of the surface.

Doulting stone is from the Inferior Oolite near Shepton Mallet. It is a shelly Jurassic Limestone, very hard and durable. It has been used in the construction of Wells Cathedral and Glastonbury Abbey, and was the material selected by Mr. Street for use in the restoration of Bristol Cathedral. It is a heavier stone and a more compact than Dundry, the specific gravity being about 2·6, and the cubic foot weighing some 130 pounds.

Bath stone is much lighter and less compact, the specific gravity being from 2·4 to 2·6, and the weight per cubic foot in pounds varying from 116 (Combe Down) to 123 (Box Hill). I have not myself made any experiments on the crushing stress which this rock is capable of resisting, but from 1,800 to 2,000 lbs. (·8 to ·9 ton) is given as the crushing load for a cubic inch of the rock. It is a very absorptive rock, taking up from 10 to 15 pints per cubic foot, or from 20 to 30 per cent. by volume.

The following notes on the Bath stones are kindly supplied by Mr. W. Brock:

"Coombe Down, near Bath, formerly had extensive quarries; but they are almost exhausted, the supply at present being very uncertain. This is a first-class stone, and has been used in some of the buildings in this city. Trinity Church, St. Philip's, is an example.

"Box Ground stone, if selected and cut into small scantling, so as to get rid of the soft places, is a good weathering stone, and well adapted for plinths, sills, quoins, and copings. The quarries are very extensive, being mostly underground.

"Corsham stone, if thoroughly dried, is a good stone. The Post Office, Small Street, was built with this.

"Westwood and Stoke quarries are more recently opened; the stone from them is well adapted for external work."

The freestone beds belong to the Great Oolite series, and vary from ten to forty feet in thickness. They are generally worked by drifts or tunnels. While it still retains the "sap" or quarry water, it is soft, and may be worked with great facility. On removal from the parent rock the blocks harden considerably; a process due, apparently, to evaporation of the water with which the stone is saturated, and the deposition of the calcareous salts it contains.

The deposition being superficial forms a kind of natural protective glaze on the rock. When once this protective glaze is removed, the durability of the stone is lessened. Hence the disastrous effects sometimes brought about by scraping a building constructed of weathered oolitic stones. Thus the durability of a stone cut to size in the quarry while the quarry water is still retained by the porous material, and then allowed to weather in the quarry, is far greater than that of a stone removed green, or in any way worked after it has lost its sap. Wren is said to have used no stone in the building of St. Paul's Cathedral that had not weathered for some years in the quarry. And in oolitic quarries may now be seen carved blocks undergoing this process, and not to be subsequently touched by the tool.

The walls of houses built of any form of limestone are apt to "sweat." To an inquiry I put to Mr. Brock, whether it is usual in this neighbourhood to use brick linings to limestone-built houses, he replies: "In all good houses, when built with limestone of any sort, the inner faces are lined with brick, to prevent moisture running down the walls during change of weather or after frosts. If the whole of the interior atmosphere is kept at 60°, this is in great measure avoided. Great complaints are made in Bath, where the houses are built of oolite, of the 'sweating' of the walls, which should also be brick lined."

Ham Hill stone, from the Midford Sands series at the base of the Inferior Oolite, is occasionally used in Bristol. It may be seen in the pillars, etc., of the Colston Hall, and in the Roman Catholic pro-cathedral. It is regarded as one of the best building stones in the West of England.

Of other stones of this (limestone) class little need be said. Magnesian limestone, used in London with such marked success in the Jermyn Street Museum, and with such conspicuous failure in the new Houses of Parliament, is only to a small extent used in Bristol. Red Mansfield stone is however employed for columns and pillars. The columns in St. Paul's Church are of this stone.

Purbeck marble may be seen in the interior of the cathedral, and other marbles, such as the Devonshire and Belgian, may be seen in the interior of Bristol and Clifton houses.

The Capital and Counties' Bank, in Clare Street, is, Mr. Brock informs me, built with a Belgian marble, which appears well adapted for the city atmosphere.

The last building stone with which I shall deal is the so-called Dolomitic Conglomerate of the Trias. The Triassic beds of our neighbourhood do not produce any building stones comparable to those found in Cheshire and Worcestershire, and of which Chester and Worcester cathedrals are built. At the base of the Triassic (Keuper) marls however there is a formation known to geologists as the Dolomitic Conglomerate, from which some building stones are obtained.

The rock is very variable in texture, the lower strata containing blocks of Mountain Limestone and Millstone Grit weighing several hundredweights, as may be seen in the new road from the Bristol Station of the Port and Pier Railway to the Downs. In such a case the material can only be used as an indirect source of the limestone or firestone. From Draycott, in the Cheddar Valley line, however a good building stone is obtained, which has been used in the construction of the newer part of the Bristol Joint Station, and for the bridge over the Floating Harbour. Fragments of limestone of irregular form, and sometimes as much as two inches in diameter, are embedded in a red paste.

The specific gravity of the Draycott stone is 2·7. A cubic foot of the rock may weigh 160 pounds. As might be expected in a rock of such irregular texture, the resistance to crushing stress is variable, two-inch cubes or prisms yielding to a load of from 6½ to 9 tons. The absorptive power is also variable, from three to eight pints per cubic foot, or from 6 to 16 per cent. by volume.

The upper beds of the Dolomitic Conglomerate are much finer in texture, and are used to a considerable extent locally, Clifton College, Emanuel Church, and a great number of houses in that part of Clifton being built of this material, which has a colour varying from light orange brown to yellow. Stones of this material have been built into the cathedral, the old walls of which would almost seem to be a piece of experimental work to test the durability of different building stones. In the churches of. St. George's, Easton, St. Mary's Portbury, Westbury, Henbury, in the Mayor's Chapel, and in Portbury Priory, local Trias of this horizon is extensively employed.

At Clevedon this rock is quarried, and is locally known as Magnesian Limestone, which must not however be confused with the true Magnesian Limestone of the Permian series, obtained at Mansfield, in Nottinghamshire.

Before leaving the subject of Bristol building stones, I may perhaps be allowed to say something about those stones which were perhaps among the earliest materials used in construction in our neighbourhood. I refer to those which form the stone circles at Stanton Drew, erected, I believe (if one may so speak of that which is, at the best, a conjecture) by the Neolithic pre-Aryan inhabitants of Britain.

Of these stones Mr. C. W. Dymond, C.E., writes (1877): "Two of the stones are New Red Sandstone,—the rock of the site; one is similar to that obtained from Dundry, four miles north-west; a few are Limestone from neighbouring quarries; and the rest—forming by far the majority—are a pebbly breccia of the Magnesian Limestone, probably brought from Broadfield Down, six miles west, or from East Harptree, six miles south." Hauteville's Quoit is also given by Mr. Dymond as composed of Limestone (*Journ. Brit. Arch. Assoc.*, Sept., 1877).

I venture to think that Mr. Dymond is in error in some of his determinations. The Quoit is certainly not Limestone but is a close brown Sandstone, somewhat resembling Sarsen. The two Mr. Dymond mentions as New Red Sandstone—the rock of the site—are in my opinion of Palæozoic age. In addition to that so marked by Mr. Dymond as Dundry, there are two or three others, notably the last stone in the N.E. avenue and the two stones in the lower tyning, marked by Mr. Dymond as breccia. The stone marked limestone in the S.W. circle is a red chert. The stones in "The Cove" are quite different from most of the others, being unaltered soft Dolomitic Conglomerate. One stone in the Great circle, and one in the S.W. circle, are of a similar nature.

With regard to the great majority of the stones, the term "breccia of the Magnesian Limestone" seems particularly unfortunate as applied to stones which are silicious throughout. For many of them I do not think we need go so far as Broadfield Down or East Harptree. They are very peculiar in character, composed of a red silicious breccia. The included fragments are many of them hollow, and contain quartz crystals; many are concentrically banded, agate fashion. I regard the rock as a Dolomitic Conglomerate, much altered by the percolation of silicious and ferruginous waters, the silica and iron of which have largely replaced the original materials (or some of them, for many of the included fragments would seem to be Millstone Grit, while others may have been originally Mountain Limestone). I propose however to return to this question, here or elsewhere, on a future occasion. The variability of the stones is remarkable.

I feel pretty confident that the source of some at least of these stones is Leigh Down, near Winford. The stone there is, on the whole, similar in character to that of the peristaliths, similarly silicious, and (if I may so say) similarly variable. On the sides of the Down lie many detached fragments, some of which seem to me to have in one place been arranged in a miniature circle. Leigh Down overlooks a little tributary of the Chew. It is distant from the Stanton Drew circles not more than 3½ miles. Hence I believe the old Neoliths brought many of the stones for the construction of their stone circles. Others are not improbably from other sources, of which East Harptree is probably one. I am engaged on an investigation of the matter.

APPENDIX.

EXPERIMENTS ON THE RESISTANCE OF SOME BRISTOL BUILDING STONES TO CRUSHING STRESS.

THESE experiments were carried out in the University College, Bristol, with the College testing machine. The primary object was to ascertain the effect of alternately freezing and thawing, soaking and drying, heating and cooling the stones. The variable resistance of the stones made these experiments of little value; still the table, as a record of practical results, may not be worthless.

The cubes and prisms were prepared by Messrs. E. G. Browne & Co., of St. Augustine's Parade. My thanks are

I.

TABLE OF RESISTANCE TO CRUSHING STRESS.

38,846

...of Comparative ...ology
JUL 20 1942

Stone.	Size of Test.	Notes.	Cracked at in tons.	Broke down at in tons.	Broke down at in lbs.	Notes.
1. Dundry . . . (1)	1″ Cube . . .	Uniformly oolitic in structure	1 ton	1·1	2,464	Irregular pyramid from base . . . 1.
2. ,, ... (2)	,, . . .	,, ,, ,, ...	—	·77	1,724·8	Fragments . . . . 2.
3. ,, ... (3)	,, . . .	Saturated with water	—	1·15	2,576	,, ... 3.
4. ,, ... (4)	2″ Cube . . .	Dry	—	3·61	8,086·4	Irregular Pyramid from base . . 4.
5. ,, ... (5)	,, . . .	,, ...	2·75	3·41	7,638·4	,, ,, ,, ,, . . . 5.
6. ,, ... (6)	,, . . .	Saturated with water	—	3·3	7,392	,, ,, ,, top . . . 6.
7. ,, ... (7)	,, . . .	Dry	—	4·13	9,251·2	,, ,, ,, base . . . 7.
8. ,, ... (8)	,, . . .	Frozen and thawed 4 times, wet . . . . +	—	2·91	6,518·4	,, ,, ,, ,, . . . 8.
9. ,, ... (9)	,, . . .	,, ,, 8 ,, ,, ... =	—	4·55	10,192	. . . . 9.
10. ,, ... (10)	,, . . .	,, ,, 8 ,, ,, ... +	1·96	3·11	6,966·4	. . . . 10.
11. ,, ... (11)	,, . . .	,, ,, 12 ,, ,, ... =	—	3·30	7,392	. . . . 11.
12. ,, ... (12)	,, . . .	,, ,, 12 ,, ,, ... =	2·96	3·42	7,650·8	. . . . 12.
13. ,, ... (13)	,, . . .	,, ,, 12 ,, dry . . . . -	—	3·83	8,579·2	. . . . 13.
14. ,, ... (14)	Prism 2″ × 3″ .	Dry . . . . -	—	4·25	9,520	Irregular wedge . . . . 14.
15. ,, ... (15)	,, ,, .	,, ...	—	3·75	8,400	,, ,, ... 15.
16. ,, ... (16)	,, ,, .	Dark irregular streak	—	3·67	8,220·8	Collapsed along streak . . . . . 16.
17. Pennant . . (1)	2″ Cube . . .	Uniformly close grained good stone	—	25·19	56,425·6	Perfect pyramid . . . . 17.
18. ,, ... (2)	,, . . .	,, ,, ,, ,, ...	—	21·28	47,667·2	,, ,, ... 18.
19. ,, ... (3)	,, . . .	,, ,, ,, ,, ...	—	19·95	44,688	. . . . 19.
20. ,, ... (4)	,, . . .	Soaked and dried 19 times. Line of weakness parallel to bedding brought out by soaking .	—	18·56	41,663·4	Irregular pyramid . . . . . . 20.
21. ,, ... (5)	Prism 2″ × 3″ .	. . . .		21·98	49,235·8	Two regular pyramids . . . . . 21.
22. ,, ... (6)	,, ,, .	. . . .	—	19·91	44,598 4	,, fairly regular pyramids . . . 22.
23. ,, ... (7)	,, ,, .	Left for 24 hours under 18 tons	—	19·5	43,700	Broke down two minutes after load 23.
24. Draycott . . (1)	2″ Cube . . .	Large, included fragments of limestone . . .	—	7·54	16,889·6	Fragments . . . . 24.
25. ,, ... (2)	,, . . .	,, ,, ,, ,, ...	—	6·3	14,112	Irregular pyramid . . . . . . 25.
26. ,, ... (3)	Prism 2″ × 3″ .	,, ,, ,, ,, ...	—	6·48	14,515 2	,, ,, ... 26.
27. ,, ... (4)	,, ,, .	,, ,, ,, ,, ...	—	9·13	20,451·2	Fragments . . . . 27.
28. Black Rock . (1)	2″ Cube . . .	Shelly and crystalline; not quite square . .	—	11·66	26,118·4	,, ... 28.
29. ,, ,, . . (2)	,, . . .	,, ,, ,, ...	8	12·48	27,955·2	Very irregular pyramid . . . . 29.
30. ,, ,, . . (3)	,, . . .	,, ,, ,, ...	7·2	12·93	28,963·2	,, ,, ,, ... 30.
31. ,, ,, . . (4)	Prism 2″ × 3″ .	,, ,, ,, ...	19	19·46	43,590·4	,, ,, ,, ... 31.
32. ,, ,, . . (5)	,, ,, .	Frozen and thawed 12 times	—	14·22	31,852·8	. . . . 32.
33. Millstone Grit . (1)	2″ Cube . . .	Strong and close-grained stone; slightly notched at base . . . . =	7·33	15·22	34,082·8	Fragments . . . . 33.
34. ,, ,, . (2)	,, . . .	Not quite square . . . . +	11·17	19·39	43,433·6	Irregular splintery pyramid . . . 34.
35. ,, ,, . (3)	1″ ,, . . .	. . . . =	10·17	13·00	29,120	Irregular pyramid . . . . . . 35.
36. ,, ,, . (1)	,, . . .	Not quite square . . . . =	10·40	10·80	24,192	Broke to fragments unexpectedly . 36.
37. Old Red . . (1)	2″ ,, . . .	Frozen and thawed 12 times . . . . . . +	—	10·70	23,968	Good pyramid . . . . 37.
38. ,, ,, ... (2)	,, . . .	,, ,, ,, ... =	7·33	12·00	26,880	,, ,, ... 38.
39. ,, ,, ... (3)	,, . . .	Close grained good stone . . . . +	7·59	9·46	21,190·4	,, ,, ... 39.
40. ,, ,, ... (4)	,, . . .	,, ,, ,, ... =	7·25	10·91	24,438·4	,, ,, ... 40.
41. ,, ,, ... (5)	,, . . .	,, ,, ,, ... +	—	9·30	20,832	,, ,, ... 41.

[*Bristol Building Stones, to face p.* 113.

due to them, and to Mr. F. W. Hardwick, Mr. W. E. Kerslake, and Mr. A. E. Machett, for their aid in the investigation.

All the experiments were carried out under the same conditions, iron being the substance used between the jaws of the machine and the test.

The details of the experiments are given with sufficient fulness in Table I. Table II. gives, not strict means, but what seem to be the average resistances as the outcome of the tests. In the last column I have given the height of a column of the rock two inches square, which would crush at the base by its own weight.

II.

TABLE OF AVERAGE RESISTANCE.

Stone.	Size of Test.	Resistance in tons.	Resistance in lbs.	Height in feet of column 2′ square which will crush with its own weight.
Dundry . . .	Two-inch cube.	3·3	7,392	2,112
Pennant . .	,,	21	47,040	10,357
Draycott . .	,,	8	17,920	4,032
Black Rock . .	,,	15	33,600	7,200
Millstone Grit .	,,	19	42,560	9,516
Old Red Sandstone	,,	11	24,640	6,037

Comparison between the results obtained with two-inch cubes and prisms two inches square and three inches high, give different results with different materials. With Dundry stone the mean of four experiments (4–7) with cubes gives 3·64 tons as the mean resistance to crushing, while the mean of three experiments (14–16) with prisms gives 3·89 tons,—a slight advantage on the side of the prism. On the other hand, with Pennant, the mean of four experiments with cubes (17–20) gives a resistance of 21·24 tons; while the mean of three experiments (21–23) with prisms gives 20·46 tons, a slight advantage on the side of the cube. It will be seen that the differences between the means in each case are far less than those between the extremes for cube and prism respectively.

With Dundry stone one-inch cubes give about one-third of the resistance of two-inch cubes.

With Millstone Grit the resistance of the one-inch cubes is out of all proportion to that of the two-inch cubes. With so hard and brittle a rock it is possible that the larger cubes had invisible cracks. The one-inch cube also indents the iron deeply, and is thus more firmly gripped in proportion than the two-inch cube.

Experiments 3 and 6 were made with the object of testing whether stones saturated with water offered more or less resistance to crushing than the dry stone. The results do not show any departure from the average of those obtained with dry prisms.

Experiments 36–41, and 31, 32, were made on rocks which had been repeatedly frozen and thawed. They do not seem to indicate any weakening.

Experiments 37–41, with Old Red Sandstone bring out clearly the fact that stones are weaker when the incidence of the stress is on the edge of the bedded layers (+), than when it is on the bed (=).

Old Red Sandstone and Pennant gave very fairly definite pyramids after fracture, with an angle of about 62° at the apex. Millstone Grit, Black Rock, and Dundry stone gave very irregular pyramids, or irregular splintery forms. In some cases these rocks, and Draycott stone in three cases out of four, went into a heap of fragments.

Cubes of Old Red and Pennant gave one pyramid generally, base downwards. Pennant prisms gave in two cases two pyramids.

The results are given, as they were obtained, for what they are worth. I am conscious that they leave much to be desired.

NOTES SUPPLEMENTAL TO THE

Flora of the Bristol Coal-Field.

1886.

By JAMES WALTER WHITE.

Arenaria leptoclados, *Guss.*

In our district this plant was first identified at Clevedon a year or two ago; and oddly enough it is cited from thence by Professor Babington, in his "Flora of Cambridgeshire," rather a curious place in which to find a Somerset record. Within the last year it has been found growing abundantly near Keynsham, and in smaller quantity at Burnham, Hallatrow, and Saltford.

Coronilla varia, *L.*

An alien, common in central Europe. It has been observed during one or two seasons in the Black Rock Quarry, and for several years on old colliery *débris* near Kingswood, West Gloucestershire; in both instances accompanied by other aliens. It is a pretty plant, bearing on long peduncles umbels of elegant lilac and white flowers.

Vicia gracilis, *Lois.*

An addition to the Flora, from the Somerset division. At the time when our Leguminosæ were published, the mention of this vetch by Babington in the "Flora Bathoniensis"—a solitary record, unconfirmed during nearly fifty years—did not seem sufficient ground for its being included. Whatever uncertainty attached to that record has been removed by Mr. David Fry, who sends me a note on his re-discovery of the plant.

The only record which has hitherto connected this vetch with the Bristol district is the following, in the "Supplement to the Fl. Bathon. 1839," p. 74. "*Ervum gracile.* On Barrow Hill." There does not appear to have been any confirmation of *V. gracilis* on Barrow Hill until September, 1886, when a careful examination of the locality disclosed the continued existence in fair, but by no means abundant, quantity of this rare species, at the spot where it was first discovered by Babington, now nearly half a century ago. *V. gracilis* may easily be mistaken for the closely allied and comparatively common *V. tetrasperma*, especially when, as in the case of the Barrow Hill plant, all the peduncles are single flowered, and none of the pods more than five-seeded. But the very differently shaped hilum of the seed affords a character whereby the two species may be satisfactorily separated. In *V. tetrasperma* the hilum is linear-oblong and much larger than that of *V. gracilis*, which is very minute and roundish-oval in outline. Dr. Boswell remarks in E. B. that "the length of the hilum appears a constant character in all the vetches," a conclusion which our own observations tend to confirm.

Epilobium lanceolatum, *S.* and *M.*

Note on its occurrence in Somersetshire, communicated by Mr. David Fry.

This is one of the rarest species of British Epilobia and has long been known at several spots near Bristol in the Gloucestershire division of the district, but had not been recorded for Somerset until it was discovered in July, 1886, on the Coal Measures at Brislington. Here, over a somewhat limited area, it occurs in considerable quantity, being associated with several of the commoner species of the same genus. One of the stations at which *E. lanceolatum* grows in Gloucestershire is on the bank of the Avon almost directly opposite the spot in Somerset where the plant has now been discovered, and on the same geological formation. Its presence, therefore, at the latter locality is not altogether surprising; and that this species has been so long overlooked at Brislington is no doubt due, in part, to the inaccessible and out-of-the-way position of the habitat.

The peculiar grey-green hue of it foliage, and (as originally remarked by Mr. Briggs) the pure white of the flowers as they open, turning rapidly to a rosy pink (not purple, as in *E. montanum*), are characters which, apart from others, serve at first sight to distinguish *E. lanceolatum* from the other Epilobia with which, in the Bristol district at least, it usually grows.

Valerianella Auricula, *D C.*

In the herbarium of the late Dr. H. O. Stephens there are specimens of several plants collected by him many years ago in the neighbourhood of Bristol, which either have escaped the observation of other botanists, or have possibly been lost. Among these is a sheet of *V. Auricula*, D C., without date, labelled "Eastwood, Brislington." It is therefore gratifying to be able to record the re-discovery of this species by Mr. David Fry last August (1886) in an arable field between Keynsham and Stockwood. It was found in considerable abundance, growing with several other interesting and uncommon plants, *e.g.*, *Specularia hybrida*, D C.; *Galium tricorne*, With.; *Papaver Lecoqii*, Lamot.; *Anagallis cœrulea*, Schreb.; *Avena fatua*, L. (var. *pilosissima*, Gray), and a single specimen of *Silene noctiflora*, L; which last, although probably only a casual, may be worth noting, as it does not appear to have been previously recorded from any part of Somerset, or elsewhere in the Bristol district.

Carduus arvensis, *Curt.*, var. *β setosus.*

By the river Avon, towards Bath, where it was observed by Mr. J. G. Baker in 1884, and subsequently by Mr. A. E. Burr, who very kindly furnished me with fresh specimens. As one of the rarest and most remarkable of British thistles, I chronicle the presence of *C. setosus* in the district with much pleasure. Classed as a variety under *C. arvensis*, the plant in this country has so little affinity for the type, that it would probably be accorded specific rank were it not that on the Continent intermediate states are said to occur, which seem to render it impossible to draw a line of demarcation between it and the common form. The leaves of our plant are soft and flat, not undulated, amplexicaul, nor at all decurrent; the sinuations shallow and beautifully spinous-ciliate, with spines so weak that the leaves can be handled with impunity. Lower petioles elongate. Anthodes long-stalked, in a long lax panicle.

Anthemis arvensis, *L.*

In fair quantity on a heap of old colliery rubbish near Kingswood, West Gloucestershire, where it has been introduced by means unknown, but now seems well established. The apparent absence of this species from the area of the Bristol Coal-field has not escaped notice, and is a peculiar circumstance. Although somewhat rare, the plant is widely distributed in England, and in some of the western counties occurs frequently among clover and sown grasses, as well as on the borders of cultivated fields.

Mentha rotundifolia, *L.*

A correction. In the "Flora," p. 136, it is mentioned that a few plants of *M. sylvestris* were found by a roadside at Portbury. These were really *M. rotundifolia*, shown by recent observation to grow there in some quantity.

Mentha sylvestris, *L.*, in Gloucestershire.

A good patch on the bank of Avon, near Hanham.

In Somerset, Mr. Fry reports a fresh locality by the Chew, near Publow; we have seen the plant also, in greater abundance, lower down the stream, at intervals, for a considerable distance.

Calamintha officinalis, *Moench*, var. *Briggsii.*

Among brambles and long grass near the Avon, under Leigh Wood. One of the most interesting of recent discoveries, as previously it had only been seen in Devonshire. I noticed this variety about two years ago, but did not feel sure of its identity until I had submitted an example to Mr. Briggs, who confirmed the name, remarking also that in his opinion the form was of small importance, and scarcely worth separating. In its extreme state, however, this plant differs widely from the type, especially in the peduncles of the lower verticillasters, which are sometimes an inch and a half long, exceeding the pedicel of the central flower of the cyme. Moreover, it is larger and lankier in all its parts, showing a marked divergence in the direction of *C. sylvatica*. *C. Briggsii* is well figured in "English Botany."

Salix acuminata, *Sm.*

See Fl., p. 172. The willow that stands under this name would more correctly be referred to *S. rugosa*, Leefe; and the latter should be classed as a species rather than as a variety of *S. Smithiana*, Willd. The name *acuminata* is now restricted to a different plant, which has not yet been found within the limits of our district.

Juncus compressus, *Jacq.*, in West Gloucestershire. Communicated by Mr. D. Fry.

This apparently rare and little understood species, which, according to Watson, has been frequently confused with the closely allied but much commoner *J. Gerardi*, Lois, was found in fair quantity last August (1886) by a roadside at Bitton, in company with *Lepigonum rubrum*, Fr., another species of very unfrequent occurrence with us. Hitherto the claim of *J. compressus* to a place in our local Flora depended solely on a specimen in the Stephens Herbarium, labelled "Horfield," a locality at which it has not recently been observed, and it is satisfactory to record that this rush does undoubtedly exist within the limits of our district. The distinguishing characters of this species are: the oval-subglobular, shortly mucronate capsules exceeding the perianths, the compressed stems, short panicle branches, and tufted habit of growth, which last peculiarity markedly separates it from its congener, *J. Gerardi*, Lois.

Calamagrostis Epigeios, *Roth.* Communicated by Mr. D. Fry.

The discovery last autumn of this handsome species at three localities in North Somerset, viz., Keynsham, near Whitchurch, and at Farrington Gurney (at the last by Miss Sherring), is remarkable, inasmuch as all that was previously known respecting its occurrence within the area of the Bristol Coal-field consisted (1) of a specimen in the herbarium of the late Dr. Perrin, of Temple Cloud, now in the possession of Mr. R. V. Sherring, F.L.S., of Hallatrow Court, and labelled "Bristol, 1838," without any further particulars; (2) of the two following records in the Flora, Part vi., p. 231; viz., "Border of Bishop's Wood, Wells; *Miss Livett.*" "By the roadside, between the Monument and Tracy Park." The latter quotation refers to a locality on the eastern border of the Gloucestershire portion of the district, where the plant does not appear to have been recently observed, and is, we have reason to fear, now extinct.

The Perrin Herbarium, referred to above, is an interesting collection in a local point of view, as it contains, in addition to Dr. Perrin's own specimens, several which were collected by the late Mr. S. Rootsey, a well-known Bristol botanist, and by one of his daughters. Among other valuable plants in this herbarium is an example, the only one we have ever seen, of the now extinct *Vicia hybrida*, Linn., which, in its hairy standard and other characters, agrees well with the description and figure in "English Botany." This specimen, although unfortunately without any particulars of locality or date, was presumably gathered on Glastonbury Tor Hill, the only spot in Great Britain from whence *V. hybrida* has ever been recorded; but both that vetch and *Vicia lutea*, which formerly grew at the same place, have long since been lost, the latter plant surviving a few years after the former had disappeared.

In conclusion, I may perhaps be allowed to refer to a matter of interest to local naturalists, and connected with the Bristol Flora, that arises from an attack made a short time ago in the columns of the *Standard* newspaper upon the "Wantonness of Botanists." The botanical fraternity throughout the kingdom were accused of pitilessly compassing the destruction of indigenous rarities by digging them up wholesale. Without touching the general question, whether we do or do not endanger the continuance of rare species by our gatherings, I would draw attention to the nature of the grounds upon which these charges were based, particularly those advanced by a correspondent hailing from Bath, under the initials "W. G. W.," whose grievance referred to two plants of our own Flora; viz., *Euphorbia pilosa* and *Dianthus cœsius.* The Euphorbia was stated to have been eradicated so completely that "W. G. W." had searched for it repeatedly without result; and, in fact, was indebted for a specimen to a root heedlessly dropped on the wayside by the retreating destroyer! Our accuser said likewise that *Dianthus cœsius* at Cheddar was in imminent danger of extinction from the same cause.

Now this is all utter nonsense, without a morsel of fact for foundation. It is a sample of baseless assertion, unhappily not rare, by which persons uninformed on the subject are induced to pass libellous judgments on the unoffending botanist.

The actual facts concerning these two plants are as follows: *Euphorbia pilosa* grows in an out-of-the-way spot, and to my knowledge "W. G. W." is by no means the only person who has looked for it in vain, although I trust he may be the only one who, on that account, concluded that it had disappeared. In the summer of 1884, at the very time when the extirpation was alleged to have been complete, I saw the plant flowering in plenty; and yesterday (June 8th, 1887), when visiting the place, there seemed to me to be a greater abundance than ever before; the clumps of stems in bloom being conspicuous at some distance. This spurge is one of the larger indigenous species. Unless the landowner grub and plough the soil, the roots are safe enough. A botanist needs them as little as he needs those of a furze-bush when he takes a specimen.

The Cheddar Pink, too, is likely to hold its ground as long as the world shall last. In addition to the numberless tufts securely situate on inaccessible ledges and in crannies of the cliffs, whence seed is scattered abundantly, the pink is plentiful in spots where any one can handle it without risking his neck. To remain ignorant of this, one must be content to walk up and down the gorge when the plant is not in flower.

The Fungi of the Bristol District.

PART X.

By CEDRIC BUCKNALL, Mus. Bac.,

Corresponding Member of the Cryptogamic Society of Scotland.

1311. LASIOSPHÆRIA FULCITA, n. sp. Plate XIII., fig. 8.*

Perithecia crowded, emergent, then superficial, ovate, rugose, sparsely clothed with short, septate, brown hairs, ·04 mm. in diameter; ostiola large, deeply radiato-sulcate; asci linear, 120 × 5; paraphyses filiform; sporidia elliptic, at first simple, then uniseptate, brown, 1–2-nucleate, 12–13 × 5.

On rotten oak branches.

The perithecia are erumpent between the fibres of the wood, sometimes at first covered by the bark, then naked and superficial. The ostiola appear to be supported by from three to six radiating buttresses. The sporidia are for a long time simple, and do not become uniseptate until after they have changed from hyaline to brown.

The following species are either hitherto unrecorded as British, or have occurred for the first time in Britain in this district:—

1241. AGARICUS (AMANITA) SOLITARIUS, *Bull. Champ. t. 48. Fr. Hym. Eur., p. 22. Grevillea XV., p. 65.*

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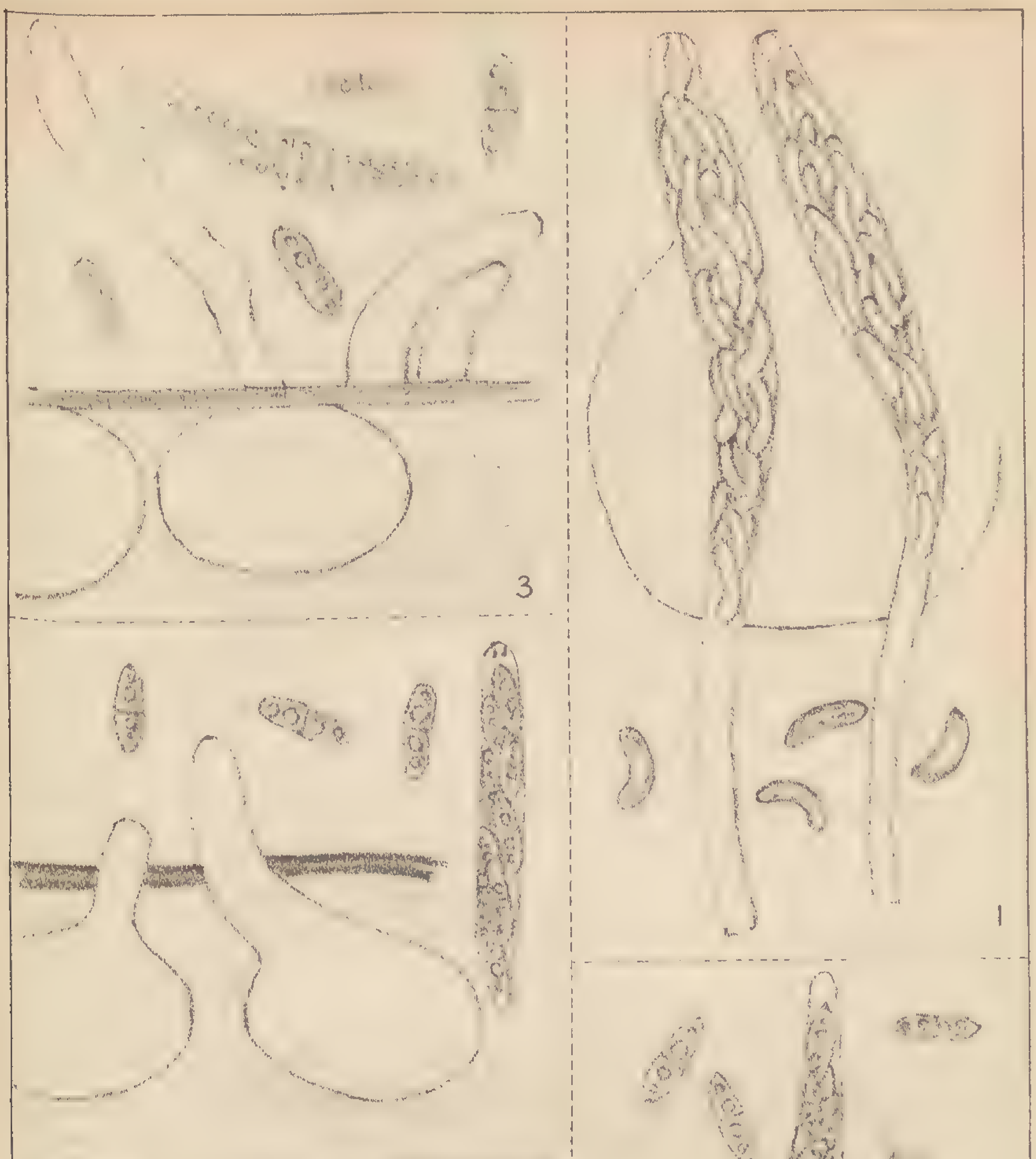
* The figures in Plates XI. to XIV. are all drawn to scale, the asci and sporidia being magnified about 830, and the perithecia 70 diameters.



1. *Cryptovalsa Nitschei, Fckl*

3. *Diaporthe discrepans, Sac*

2. [illegible]

4. [illegible] *Fckl*

1274. Hydnum fusco-atrum, *Fr. Hym. Eur., p. 612. Grevillea XV., p. 67.*

1299. Cryptovalsa Nitschei, *Fckl. Sacc. Syl. No. 692.* Plate XI., fig. 1.

1300. Diaporthe (Euporthe) Tulasnei, *Nke. Sacc. Syl. No. 2526.* Plate XI., fig. 2.

On nettle stems.

1301. Diaporthe (Euporthe) discrepans, *Sacc. Syl. Add. No. 6098.* Plate XI., fig. 3.

On stems of Rumex.

This agrees well with the description of the *Diaporthe* on Rumex from North America, and also with that of *D. orthoceras*, which is described as only growing on *Compositæ*. As the genus *Diaporthe* is to a great extent classified with reference to the host plants, I am bound to refer my specimens to the former species.

1302. Diaporthe (Tetrastaga) Quercus, *Fckl. Sacc. Syl. No. 2572.* Plate XI., fig. 4.

On oak branches.

1305. Diaporthe (Tetrastaga) crustosa, *Sacc & Roum. Syl. No. 2603.* Plate XII., fig. 5.

On holly branches.

1307. Lophiostoma (Schizostoma) montellicum, *Sacc. Syl No. 5398.* Plate XII.,. fig. 6.

This agrees better with Saccardo's description than with his figure, *Fung. Ital. No. 146.* It evidently belongs to the group comprising *S. montellicum*, *S. vicinum*, and *S. vicinellum*, which appear to differ principally in the size of the spores. The relative proportions of my specimens agreeing best with *S. montellicum*, I refer it to that species.

1309. Lophiostoma vagans, *H. Fabre, Sacc. Syl. No. 5483.* Plate XII., fig. 7.

On a stick of mahogany.

The sporidia agree well with the figure in *Spher. Vaucl. fig. 48–49*, but it may be the same as *L. pseudomacrostomum.*

1317. Sphæria (Apiospora) Montagnei, *Dur. & Mont. Sacc. Syl. No. 2098.* Plate XIII., fig. 10.

On Pampas grass.

1318. Sphæria (Leptosphæria) galiorum, *Sacc. Syl. No. 2928.* Plate XIV., fig. 12.

On *Galium.*

1319. Sphæria (Leptosphæria) culmicola, *Fr. Sacc. Syl. No. 3110.* Plate XIV., fig. 13.

On stems of *Phragmites communis.*

0. Sphæria (Leptosphæria) dolioides, *Auers. Sacc. Syl. No. 3011.* Plate XIV., fig. 14.

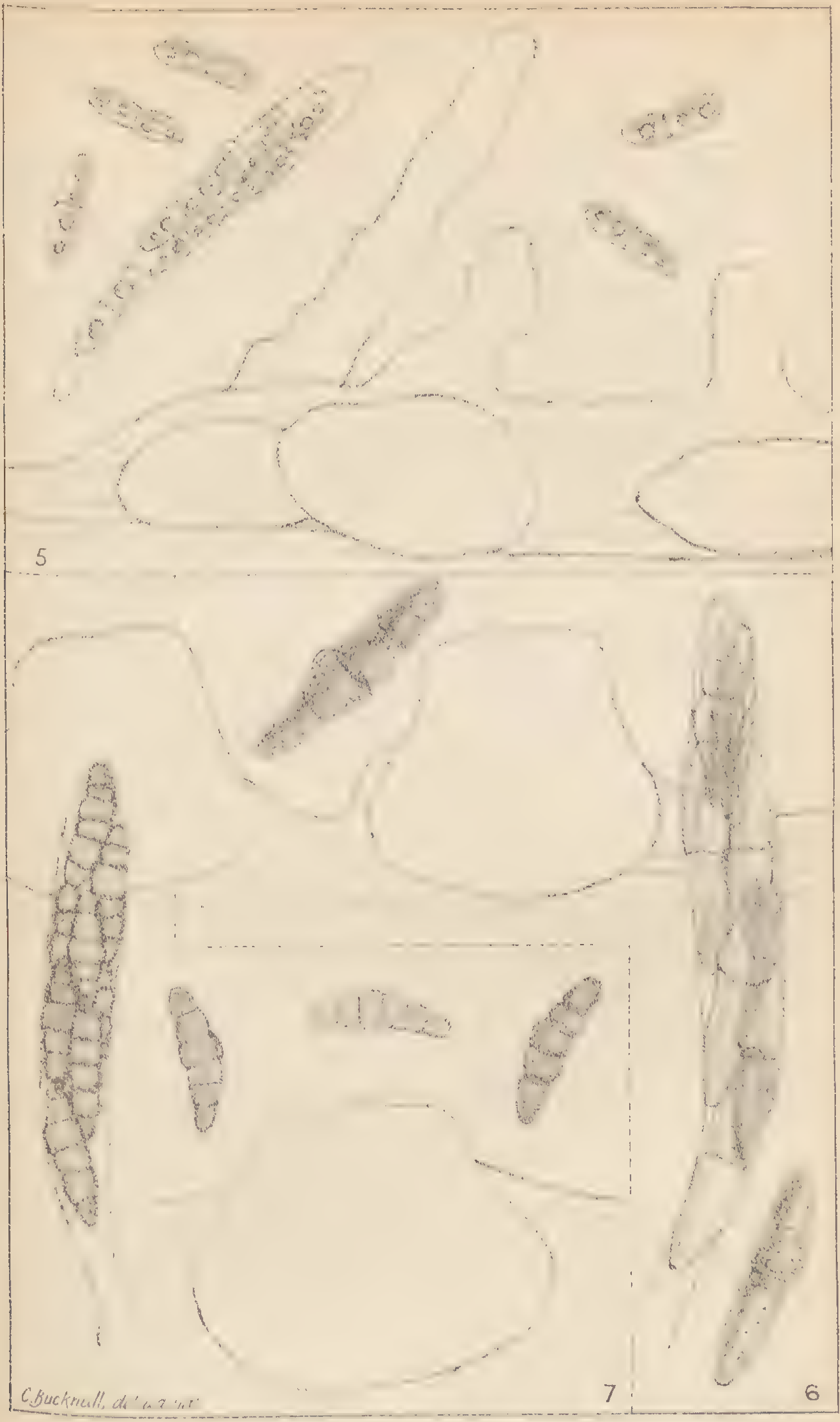
On stems of *Tanacetum vulgare.*

1321. Sphæria (Metasphæria) culmifida, *Karst. Sacc. Syl. No. 3476.* Plate XIV., fig. 15.

1241. Agaricus (Amanita) solitarius, *Bull.*	Leigh Woods,	Sept.,	1886.
1242. Agaricus (Lepiota) clypeolarius, *Fr.*	Blaise Castle Woods,	Nov.,	,,
1243. Agaricus (Lepiota) cepæstipes, *Sow.*	Clevedon,	May,	1887.

In a greenhouse. Communicated by Mr. E. Wheeler.

1244. Agaricus (Tricholoma) vaccinus, *Pers.*	Brockley Coombe,	Nov.,	1886.
1245. Agaricus (Clitocybe) amplus, *Pers.*	Blaise Castle Woods,	,,	,,
1246. Agaricus (Collybia) conigenus, *Pers.*	Near Sea Mills,	May,	1887.
1247. Agaricus (Mycena) pelianthinus, *Fr.*	Blaise Castle Woods.	Nov.,	1886.
1248. Agaricus (Mycena) ammoniacus, *Fr.*	Kingsweston,	Oct.,	,,
1249. Agaricus (Mycena) stanneus, *Fr.*	Durdham Downs,	Nov.,	,,



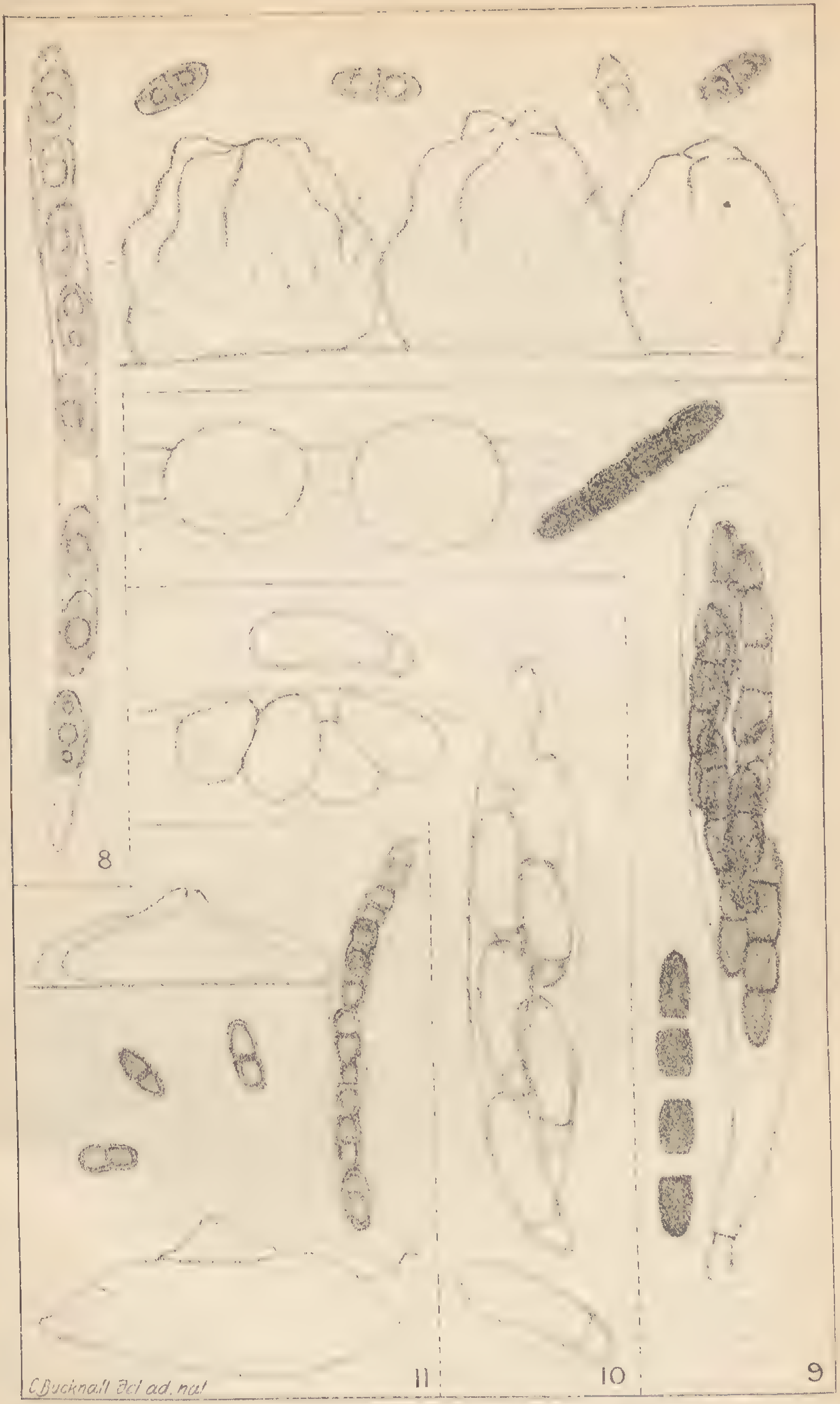
5 *Diaporthe crustosa*, Sacc. & Roum.

6 [illegible] 7 [illegible]

1250. Agaricus (Mycena) vulgaris, *Pers.*	Brockley Coombe,	Nov.,	1886.
1251. Agaricus (Omphalia) hydrogrammus, *Fr.*	”	”	”
1252. Agaricus (Leptonia) asprellus, *Fr.*	Leigh Woods,	Oct.,	,,
1253. Agaricus (Flammula) lentus, *Pers.*	Blaise Castle Woods,	Nov.,	,,
1254. Agaricus (Flammula) lubricus, *Fr.*	Westridge Wood,	Sept.,	1880.
1255. Agaricus (Psathyra) bifrons, *B.*	Durdham Downs,	Oct.,	1886.
1256. Agaricus (Psathyrella) aratus, *B.*	”	”	”
1257. Agaricus (Psathyrella) hydrophorus, *Bull.*	Beaufort Road,	April,	1878.
1258. Coprinus fimetarius, *var.*, pullatus, *Bolt.*	Osborne Road (Mr. L. Rogers),	May,	1887.
1259. Cortinarius (Phlegmacium) cyanopus, *Fr.*	Leigh Woods,	Sept.,	1886.
1260. Cortinarius (Phlegmacium) scaurus, *Fr.*	Durdham Downs,	,,	1884.
1261. Cortinarius (Myxacium) emollitus, *Fr.*	Kingsweston,	Oct.,	1886.
1262. Cortinarius (Dermocybe) camurus, *Fr.*	Blaise Castle Woods,	Nov.,	,,
1263. Cortinarius (Telamonia) bivelus, *Fr.*	Abbott's Leigh,	Sept.,	1885.
1264. Cortinarius (Hydrocybe) privignus, *Fr.*	Blaise Castle Woods,	Nov.,	1886.
1265. Cortinarius (Hydrocybe) scandens, *Fr.*	”	”	”
1266. Hygrophorus hypothejus, *Fr.*	Coombe Hill,	,,	”
1267. Lactarius pyrogalus, *Bull.*	Blaise Castle Woods,	Sept.	,,

1268.	Lactarius chrysorheus, *Fr.*	Blaise Castle Woods,	Nov., 1886.
1269.	Russula fellea, *Fr.*	Stapleton,	,, ,,
1270.	Polyporus fumosus, *Fr.*	Brockley Coombe,	,, ,,
1271.	,, spumeus, *Fr.*	Leigh Woods,	,, ,,
1272.	Trametes mollis, *Sommf.*	,,	April, 1887.
1273.	Dædalea confragosa, *Pers.*	Abbott's Leigh,	Sept., 1886.
1274.	Hydnum fuscoatrum, *Fr.*	Leigh Woods,	Nov., 1886.
1275.	Phlebia merismoides, *Fr.*	,,	,, ,,
1276.	,, radiata, *Fr.*	Leigh Woods, (Mr. E. Wheeler)	— ,,
1277.	Thelephora Sowerbei, *B.*	Berwick Wood,	Aug., 1880.
1278.	Tremella tubercularia, *B.*	Leigh Woods,	Dec., 1877.
1279.	Phallus impudicus, *Linn.*	Coombe Hill,	Sept., 1886.
1280.	Lycoperdon pusillum. *Fr.*	Durdham Downs,	— ,,
1281	Craterium vulgare, *Ditm.*	Leigh Woods,	Nov., ,,
1282.	Stemonitis ferruginea, *Ehr.*	,,	June, 1884.
1283.	Phoma melæna, *Fr.*	Coombe Hill,	May, 1887.
	On *Astragalus glycyphillos.*		
1284.	Pestalozzia lignicola, *Cke.*	Durdham Downs,	June, 1886.
1285.	Puccinia mixta, *Fckl.*	,, (Mr. L. Rogers),	,, ,,
1286.	Fusisporium fœni, *B. & Br.*	Black Rock Quarry,	May, 1887.
1287.	Ascophora mucedo, *Tode.*	Clifton,	Sept., 1886.
1288.	Peziza fibrillosa, *Curr.*	Durdham Downs,	Nov., ,,
1289.	,, inflexa, *Bolt.*	The Avon, S.,	Sept., ,,
1290.	Patellaria parvula, *Cke.*	Durdham Downs,	April, 1887.

Plate XIII



C.Bucknall del ad. nat

8 *Leptosphaeria ciliata, Bucknall*   10 *Apiospora Montagnei, Dur. & Mon*

	* Hysterium curvatum, *Fr.*	Leigh Woods.	

Very common on rose and bramble stems. It was referred to *H. Rousselii* at p. 349, vol. II., Part III.

1291.	Actidium hysterioides, *Fr.*	Coombe Hill.	— —
	* Perisporium vulgare, *Corda.*	Black Rock Quarry.	May, 1885.

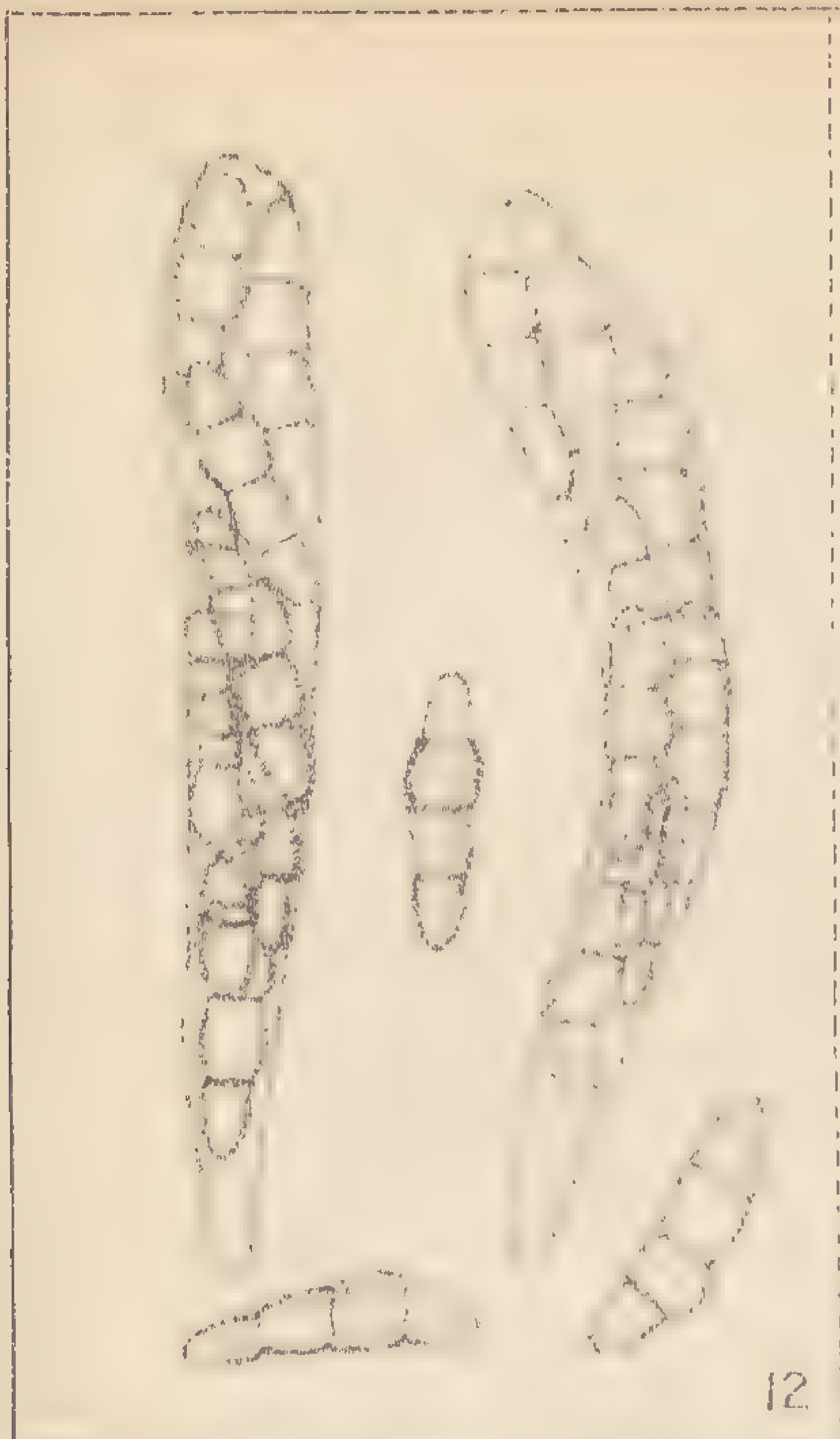
This was described and figured in error as *Sporormia secedens* at No. 1218, Vol. V., Plate V., fig. 3. A form has occurred on a piece of dead wood, apparently elm, with asci of a very different form, but I cannot refer it to any other species. It differs from the ordinary form in the smaller, crowded, generally emergent perithecia, 250–300 μ in diameter, and the larger, clavate asci, which are 120 × 15. Sporidia 36 × 7. It is figured at Plate XIII., fig. 9.

1292.	Diatrype bullata, *Fr.*	Sandy Lane,	June, 1887.
1293.	Valsa cornicola, *Cke.*	The Avon,	April, 1885.
1294.	,, (Chorostate) aceris *Fckl.*	Hanham,	Nov., ,,
1295.	Valsa (Calospora) detrusa, *Fr.*	Blaise Castle,	Spring, ,,
1296.	Valsa (Calospora) Innessii, *Curr.*	Durdham Downs,	— 1884.
1297.	Melanconis alni, *Tul.*	Ham Green.	— —
1298.	,, thelebola, *Fr.*	,,	— —
1299.	Cryptovalsa Nitschei, *Fckl.*	Leigh Woods,	June, 1885.
1300.	Diaporthe (Euporthe) Tulasnei, *Nke.*	Portbury,	June, 1879.
1301.	Diaporthe (Euporthe) discrepans, *Sacc.*	Black Rock Quarry,	Feb., 1887.
1302.	Diaporthe (Tetrastaga) Quercus, *Fckl.*	Leigh Woods.	— —
1303.	Diaporthe (Tetrastaga) sarothamni, *Awd.*	Hanham,	Nov., 1885.
1304.	Diaporthe (Tetrastaga) discutiens, *Berk.*	Markham Bottom,	May, 1887.
1305.	Diaporthe (Tetrastaga) crustosa, *Sacc & Roum.*	Black Rock Quarry,	April, 1886.

1306.	Diaporthe (Tetrastaga) epilobii, *Cke.*	Markham Bottom,	May, 1887.
1307.	Lophiostoma (Schizostoma) montellicum, *Sacc.*	The Avon,	April, 1880.
1308.	Lophiostoma (Lophiotrema) sexnucleatum, *Cke.*	,,	Sept., 1886.
1309.	Lophiostoma vagans, *H. Fabre.*	Black Rock Quarry,	Feb., 1887.
1310.	Lasiosphæria scabra, *Curr.*	Durdham Downs,	April, ,,
1311.	,, fulcita, *Bucknall p.* 126 *ante.*	Leigh Woods, Blaise Castle Woods,	— 1878. — 1884.
1312.	Psilosphæria mammæformis, *Pers.*	Durdham Downs,	Mar., 1885.
1313.	Psilosphæria dioica, *Moug.*	Coombe Hill,	May, 1887.
1314.	Conisphæria pæcilostoma, *B. & Br.*	Coombe Hill,	May, ,,
1315.	Sphæria (Anthostoma) Plowrightii, *Niessl.*	Hanham,	Nov., 1885.
1316.	Sphæria (Apiospora) Montagnei, *Dur. & Mont.*	Tyntesfield,	July, —

* Sphæria (Didymosphæria) conoidea, *Niessl.* No. 1220. *ante*, Plate XIII., fig. 11.

1317.	Sphæria (Leptosphæria) galiorum, *Sacc.*	The Avon,	June, 1885.
1318.	Sphæria (Leptosphæria) culmicola, *Fr.*	St. Philip's Marsh,	July, 1882
1319.	Sphæria (Leptosphæria dolioides, *Auers.*	Keynsham,	,, 1886.
1320.	Sphæria (Metasphæria) culmifida, *Karst.*	Black Rock Quarry,	Jan., ,,
1321.	Gnomonia vulgaris, *Ces. & De No.*	Durdham Downs,	Feb., ,,



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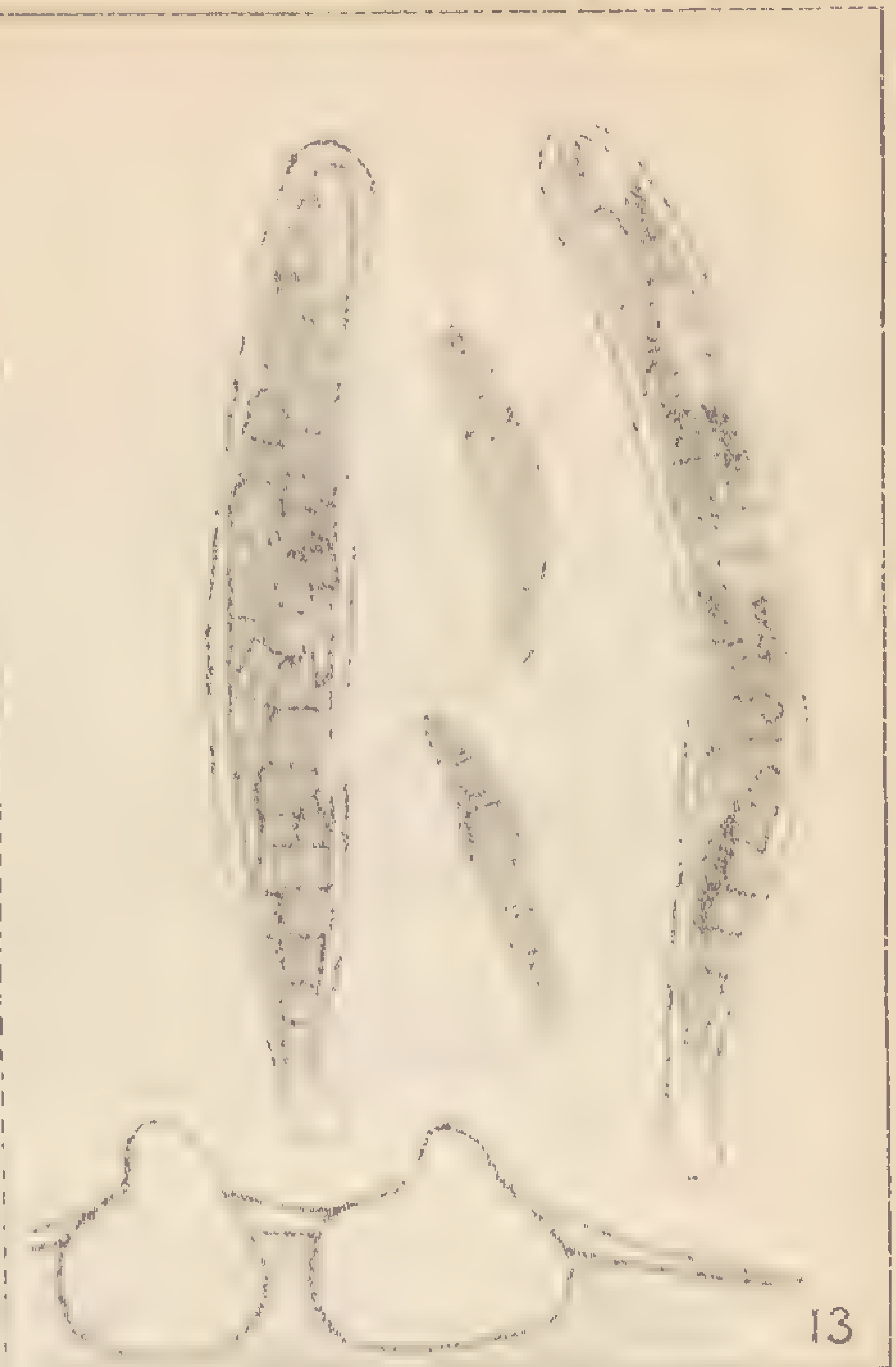

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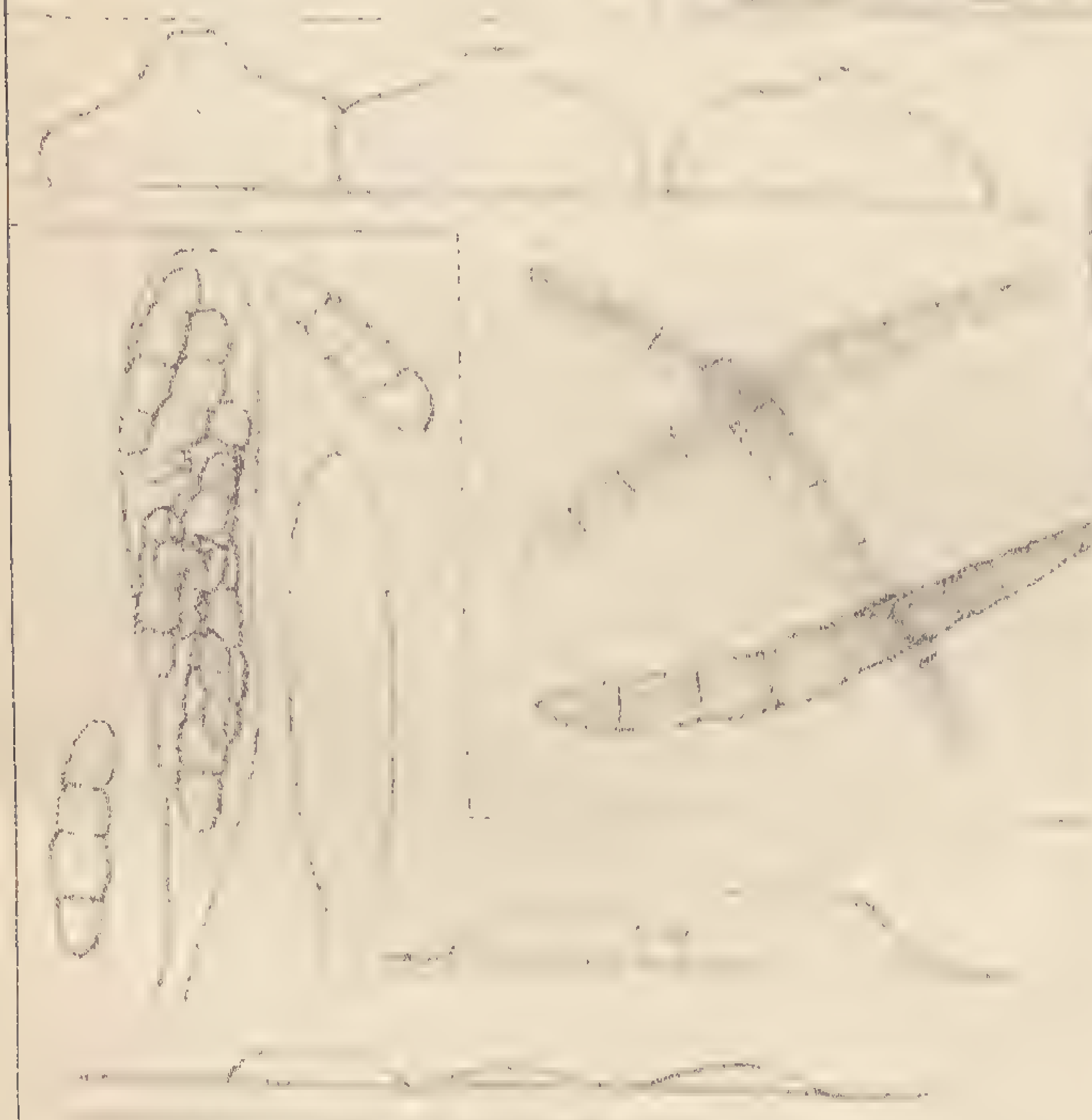

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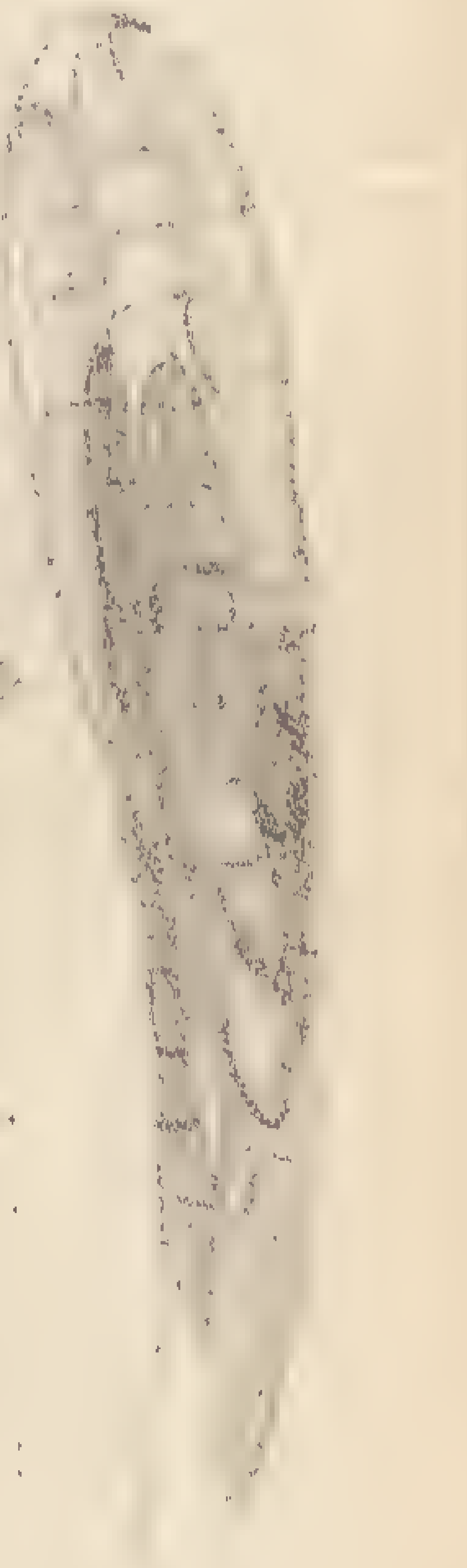

14

12. *Leptosphaeria galiorum*, Sacc. 14 *Leptosphaeria* [illegible]

Notes on the Reptiles, Amphibia, AND Fish of the Bristol District.

By H. J. CHARBONNIER.

THE first thing that strikes one on beginning the study of our local reptiles is the small number of the British species.

Whereas of 623 European birds, the British list contains 363, more than half—and of 175 species of European mammals, there are sixty-six British species, rather more than one-third,—of European reptiles, of which there are, including the amphibia, eighty-eight species, only twelve belong to Great Britain, only one-eighth of the number.

Doubtless their limited means of locomotion have tended to isolate our reptile fauna; the barrier of the sea, and the gradually more unfavourable conditions of marsh lands being drained and reclaimed, the surface drainage arising from the sinking of mines and other similar causes, all seem to tend to their eventual extinction.

Some reptilian orders, as the Testudinata (tortoises), of which there are eight European species, are quite absent.

Of the Saurians (lizards), only three species out of thirty-one occur.

Of the Ophidia (snakes), three out of twenty-four.

Of the Amphibia, six out of twenty-five.

The Saurians seem to find the temperature too low; the species getting more plentiful and the specimens of our own species larger by degrees as one travels southwards.

The Ophidians seem to diminish in proportion as the Saurians and Amphibia—on which they depend largely for food—become scarcer.

The Amphibians, finding the necessary conditions permanent in many lakes and ponds, seem to have fared the best, and we still retain one-quarter of the number of the continental species.

The tendency to variation seems less in reptiles than in other classes of vertebrata; for whilst we have species of mammals and birds which are peculiar to our isles, there is no species of reptile peculiar to Great Britain.

Notwithstanding this, I feel sure that close observation will show that some are sub-specifically different from their European congeners,—their complete isolation makes it almost certain.

Beginning with the Saurians (or lizards), of three British species we have two in our district.

1. *Zootoca vivipara*, the pretty little lizard common in all suitable localities, hot, dry, and sandy places, amongst furze and heath, at Leigh, and Brockley, and on our Downs. This active little species (varying from 5½ to 6½ inches in length) may be seen, in hot sunny weather, basking in the heat, or darting at the small coleoptera which make its principal food.

This species, like the next, is ovoviviparous, the thin membrane of the egg bursting at the moment of birth.

2. *Anguis fragilis*, the Slow-worm or Blind Worm. Notwithstanding its snake-like appearance, this is a true lizard; this also is common with us in fields and hedgerows, growing from ten to twelve or even to fourteen inches in length. They feed extensively on the smaller kinds of slugs, which they seize across the back, like a terrier does a rat. They are ovoviviparous, and have from seven to twelve young, eight or nine being the most frequent number.

Turning next to the Ophidia (or snakes), of three British species two occur with us.

1. *Tropidonotus natrix*, the Common Snake. This is fairly common in marshy grounds, feeding principally on toads and frogs. I have found three good-sized frogs and one toad in one snake; they seem to seize the unfortunate amphibians by the hind limbs, and swallow them in that way. I have never seen any specimens longer than three feet, though they reach to four feet on the Continent. The female is generally larger than the male, they lay fifteen to twenty eggs, rather larger than a sparrow's, generally in dung heaps or similar places. These snakes possess anal glands, similar apparently to the stoat and weasel, giving off an equally (or worse) offensive effluvium. Gilbert White in his "Selborne," states that the scent is emitted, "se defendo," the reptile being quite innocuous when not irritated or frightened. This snake is an expert swimmer.

It is eaten by the peasantry in the South of France as "anguilles de hâie."

2. *Pelias berus*, the Viper or Adder. The colour of this species is very variable, sometimes almost black. It reaches a length of about two feet. It is not uncommon on dry and heathy places, as at Brockley Coombe, where it seems to feed largely on the lizards. This snake is ovoviviparous. This is our only venomous species. I have heard of several

fatal cases resulting from its bite, one of a boy, and another of a horse, who happening to lie down on the reptile was bitten on the shoulder. It is said to be innocuous in winter, if roused from its torpor and made to bite, the poison seeming inert at that season.

We next come to the Amphibians, a class differing very widely from the true reptiles, though in appearance very similar, the popular mind uniting "efts" and "lizards" as being very nearly akin.

Of the six British Amphibians five occur with us, the exception being the "Natterjack Toad," a very local species.

1. *Rana temporaria*, the Common Frog, called "temporaria" on account of the dark mark on the temples. This species is common on all marshy grounds, though not as abundant as formerly; it is too well known to require any account of its history.

2. *Bufo vulgaris*, the Common Toad, also too well known to require any description. Very abundant.

I have noticed that the males are constantly rather smaller in size and darker in colour than the females. It is remarkable how the specimens increase in size as one travels southward, being larger in the Channel Islands than with us, larger still in France, until in the Morea and in Sicily they attain to a length of ten inches.

It is an almost cosmopolitan species, occurring in Central Asia, China, and Japan, and even in the Himalayas to a height of 10,000 feet.

We next come to those amphibians where the fish-like character is more apparent, the "newts" or "efts." In these the tail is retained through life, and the gills are retained until long after the four limbs are developed.

The extraordinary reparative power apparent in the higher reptiles, enabling the lizard to grow a new tail when that very brittle member gets broken, becomes still more notable in the newts; the loss of a limb, or even of an eye, or a considerable portion of the head, being soon made good by the growth of new parts.

The three British species of newts all occur in our district.

1. *Triton cristatus*, Great Water Newt. This is a handsome species, attaining to six inches in length, and is fairly common in ponds and ditches.

The tadpoles are extremely voracious. I have seen one swallow a brother tadpole, nearly his own size, and swim about with the latter's tail hanging out of his mouth. After a while, however, the victim, possibly threatening indigestion, was turned out again—this new Jonah seeming none the worse.

2. *Lophinus punctatus*, the Smooth Newt. Very common in every pond and ditch. The tadpoles of this species leave the water as early as the end of July, having by this time lost their gills, and being in fact miniatures of the adult ones, about one inch in length. They then crawl about under stones, and among grass, etc., in damp places; finally hybernating in an old stone wall, or similar place, sometimes at a considerable distance from the water.

3. *Lophinus palmatus*, the Palmated Newt. This species is said in Cooke's "Our Reptiles," to be *local*. He gives Edinburgh, Letton in Herefordshire, Bridgwater, and Dartmouth, as localities. I have found it fairly plentiful in the Somersetshire part of our district, and in ditches and ponds at Keynsham. It is a pretty little species, and in the spring the males are beautifully marked and crested. This species spawns rather later than the preceding.

The tadpoles resemble those of the preceding species, but have a more distinct crest and a broader tail; they also leave the water much later in the season, retaining their gills till the end of September.

They are very voracious, feeding at first on cyclops and various entomostraca; and later on on the larvæ of gnats. They are very pretty objects, fish-like in form, and with bright golden eyes. They are about an inch long when they leave the water in autumn.

Cooke states that, *T. cristatus* is three years in attaining its adult size, and does not return to the water till then.

Dr. Wright says, "They attain to their full size by the ensuing spring, growing rapidly during the autumn and winter." This last I cannot believe, as the winter is a time of more or less complete torpor, and cannot be a period of growth.

On the other hand, I have never been able to find specimens of intermediate size at any time of the year; those I have found in the ponds in spring are always adult, or nearly adult size,—and those that I have found hybernating have also been of adult size. I should very much like to hear if any of our members have been more fortunate.

We next come to the Fishes. Of fifty species of British freshwater fishes, I am only able to record twenty. I am sure others must occur, but I have not been able to determine them.

1. *Perca fluviatilis*, the Perch. This, one of the most beautiful of our native fishes, is abundant in the Avon, Froome, Chew, and other suitable streams in the district. The power of retaining life after severe injuries, and of repairing such injuries, mentioned with reference to the reptiles, seems still greater in fish—the perch, carp, and tench being notably so.

"Pennel," in his *Angler Naturalist*, relates the following:

"Whilst fishing in Windermere, in removing the hook from the jaws of a perch, one eye was displaced, and remained adhering to it. I returned the maimed perch, which was too small for the basket, to the lake, and being scant of minnows, threw the line in again, with the eye attached as bait; the float disappeared almost instantly, and on landing the new comer, it turned out to be the fish I had the moment before thrown in, and which had thus been caught with his own eye!"

2. *Cottus gobio*, the Miller's Thumb, or River Bullhead. This little species, three or four inches in length, occurs in most of the small streams and brooks in our neighbourhood, most often under and amongst stones.

3. *Gasterosteus leiurus*, the Smooth-tailed Stickleback. There are six species of British sticklebacks, of which however I have only been able to obtain this one,—which swarms in the rivers and brooks in our district. They live indifferently in fresh, brackish, or even salt water, in streams or in ponds. They are very voracious, and are said to be very destructive to the ova of other fish. The sticklebacks are remarkable as being the only one of our fishes, as far as I know, that display any care or affection for their progeny. The male stickleback not only building a nest and watching over the eggs, but also doing battle in defence of the young fry until they are able to shift for themselves; a degree of care and affection very extraordinary in a fish. Their nest-building habits make them great favourites in aquaria.

4. *Cyprinus carpio*, the Common Carp. Occurs in the Avon, where it used to be caught of large size, but not of late years. This species is most abundant in the southern and eastern counties, getting scarcer in the north and in Scotland and Ireland.

5. *Gobio fluviatilis*, the Gudgeon. This species is common in the Avon and its tributaries, sometimes being caught in immense numbers.

6. *Tinca vulgaris*, the Tench. Occurs in ponds in the district; in most cases it has, however, been introduced. It occurs also in the Avon above Bath, but is very rare in its lower reaches. It is a common fish in Holland, and is said to have been first introduced into England in 1514, and was kept in large quantities by the monks.

It is extraordinarily tenacious of life, and will survive most extensive injuries.

7. *Leuciscus vulgaris*, the Dace. Common in the Avon and its tributaries; it is a very bright and graceful fish, and a great addition to an aquarium. Its movements are as quick and lively as those of a trout, and its bright and silvery scales make it an especially attractive object. One I had, though extremely shy and wild at first, soon became very tame, and would take any number of flies from the fingers of those it knew.

About the beginning of April they ascend the Chew in great numbers. They spawn in May and June.

8. *Leuciscus rutilus*, the Roach. Very common in the Avon and the Froome and Chew; some are sometimes caught of large size in the Avon.

9. *Leuciscus cephalus*, the Chubb. Occurs in the Avon, but more common above Kelston than below; sometimes of good size, six to seven pounds.

10. *Leuciscus alburneus*, the Bleak. Fairly abundant in the Avon; a bright, lively little fish, seldom exceeding six inches in length. It is rarely molested except for bait; the silvery pigment underneath the scales used to be used in commerce for making artificial pearls, and vast numbers were caught for this purpose. Fortunately for this little fish

another species is now laid under contribution for this purpose, namely, the "Argentine," caught in the Tiber.

11. *Leuciscus phoxinus*, the Minnow. This universal favourite occurs in shoals in the Avon; it is rarely found exceeding three inches in length.

12. *Cobitis barbatula*, Bearded Loach. Captain Newnham has informed me that he has taken this fish in the streams in Ashton Meadows, and I have since found it myself in streams (under stones), at Keynsham.

13. *Esox lucius*, the Pike or Jack. This wolf among fishes occurs commonly in the Avon, and used to be caught of large size, but not of late years; its prodigious voracity is well known. It is a cold country fish, and reaches its perfection in Lapland and Scandinavia, attaining there to a length of five feet.

14. *Salmo fario*, the Trout. The handsomest of our freshwater fishes; is fairly common in the Avon and its tributaries, and is not unfrequently caught of good size, six or seven pounds and over.

15. *Salmo salar*, the Salmon. I hardly know if I ought to include this species, as it occurs no nearer than the Severn, which lies somewhat outside our district. I am informed, however, that it has occurred accidentally as high up the Avon as the Suspension Bridge.

16 and 17. *Anguilla acutirostris*, the Sharp-nosed, and *A. latirostris*, the Broad-nosed Eel. Both these species occur abundantly in the Avon and other streams; the former is caught in immense numbers at the weirs on the Chew when migrating, as many as a couple of tons being taken in a night; there are generally some "broad-noses" among them, but these, though they roam up and down the rivers, particularly at night, do not seem to migrate like the others.

18. *Petromyzon marinus*, the Sea Lamprey. Ascends the Severn in large numbers in April and May. I have never heard of its being found in the Avon.

19. *Petromyzon fluviatilis*, the River Lamprey. This species is common in the Severn, and is indigenous.

20. *Platessa flesus*, the Flounder. Is abundant in the Avon and Chew. Colonel Montagu recorded its occurrence in the Avon as high as within three miles of Bath.

In conclusion, I very much regret that I have been unable to make these notes more complete. I think it is a pity that whilst the insects, the shells, the plants, down to the humblest fungi, have found such able exponents amongst us, the reptiles and fish have so far been completely neglected. There exists a common prejudice against the former class, which however would disappear on investigation of their structure and habits, and there are many most interesting points in their life history on which our books are either silent or vouchsafe but doubtful information. May these incomplete notes induce some member who has more leisure to take up these branches of our vertebrate fauna, and to thoroughly work them out.

Rainfall at Clifton in 1886.

By GEORGE F. BURDER, M.D., F.R. Met. Soc.

TABLE OF RAINFALL.

	1886.	Average of 33 years.	Departure from Average.	Greatest fall in 24 Hours.		Number of days on which ·01 in. or more fell.
				Depth.	Date.	
	Inches.	Inches.	Inches.	Inches.		
January . .	4·923	3·303	+1·620	0·958	30th	24
February .	0·819	2·386	−1·567	0·410	28th	9
March . .	2·814	2·162	+0·652	0·753	30th	14
April . . .	2·347	2·075	+0·272	0·652	7th	14
May . . .	4·675	2·369	+2·306	1·069	12th	19
June . . .	1·017	2·649	−1·632	0·405	1st	7
July . . .	3·689	2·942	+0·747	0·798	25th	17
August . .	2·537	3·494	−0·957	0·956	13th	6
September .	2·683	3·393	−0·710	0·682	27th	13
October . .	4·942	3·724	+1·218	1·320	15th	21
November .	4·076	2·992	+1·084	0·715	5th	15
December .	5·182	2·826	+2·356	1·140	26th	21
Year . . .	39·704	34·315	+5·389	1·320	Oct. 15th	180

Remarks.—From the foregoing table it will be seen that the total rainfall of the past year exceeded the average by upwards of five inches. This excess was largely due to the rainy character of the last three months of the year. January and May were also very wet months. On the whole, eight of the months yielded an excess, and four a deficiency of rain. The driest of all was February, with less than an inch; the wettest was December, with over five inches. June, August, and September were dry months, and a drought of 27 days' duration, commencing in June, extended to the 11th of July. On the whole, therefore, the summer was very fine.

The year was free from any extraordinary diurnal rainfall, the heaviest being 1·320 in., occurring with a violent gale on the 15th of October. May and December also had each a diurnal fall exceeding one inch, the latter instance including the deep snow of December 26th. On that occasion the rain and melted snow amounted together to 1·140 in., and the average depth of snow lying on the ground was 4½ inches. It was the deepest snow experienced here since January, 1881, and the quantity was greater than the depth alone would indicate. Falling on a wet ground, and with a temperature rather above the freezing point, it lay compactly, and the 4½ inches of actual measurement represented not less than eight inches of dry snow. In localities a trifle colder than our own, the actual depth corresponded more nearly with the calculated depth.

The havoc wrought by this snowstorm upon the telegraph wires and poles was probably without precedent, and the circumstances conducing to this result may be worth considering. The storm was the outcome of a cyclonic disturbance which pursued a somewhat unusual course, its centre passing up the English Channel just off our southern coasts. The southern parts of England were therefore situated a little to the north of the storm-centre, instead of being, as more frequently happens, far to the south of the centre. So long as the storm-centre was distant in the south-west, the wind here was—in accordance with a well-known law—south-east, and rain was falling heavily. As the storm advanced up the Channel and passed to the south of us, the wind, instead of *veering* towards the south and west, as it would under other circumstances have done, *backed* to the east and north-east. This change of wind brought the temperature down to near the freezing point, and the rain turned to snow. The snow, falling in large moist flakes, partly melting as it fell and afterwards freezing, would cling to the telegraph wires and incrust them with ice, perhaps in some cases even cementing a whole system of wires into a mass of icy snow. A gale following under these circumstances sufficiently accounts for the destruction occasioned.

Meteorological Observations,

AS REGARDS

Temperature, taken at Clifton, 1886.

By H. B. JUPP, M.A.

THE observations from which the following tables are derived have been taken at 8.30 a.m. daily. The Thermometers used, with the exception of that for the Ground Temperature, are kept in a Stevenson cage; and in all the readings the requirements of the Royal Meteorological Society are strictly complied with.

1886 TEMPERATURES.

MONTH.	Maximum in Shade.		Minimum in Shade.		Mean in Shade.	Minimum on Ground, Lowest recorded.
	Highest recorded.	Mean.	Lowest recorded.	Mean.		
January .	48·1	38·8	24·4	32·2	35·5	20·3
February .	49·2	41·4	24·1	30·3	35·8	20·8
March . .	62·3	45·8	21·7	34·5	40·1	18·1
April . .	70·9	53·0	31·9	40·5	46·4	25·8
May . . .	72·0	60·6	35·0	45·1	52·8	30·1
June . .	80·2	66·7	45·9	51·0	58·8	41·9
July . . .	83·1	69·9	49·1	55·4	62·7	45·0
August .	68·7	63·5	46·8	54·6	59·1	43·0
September	83·5	65·1	41·6	51·2	58·2	34·5
October .	72·4	58·4	36·2	47·5	52·9	31·1
November	57·0	51·6	34·1	42·6	47·1	20·3
December.	53·2	44·0	21·8	33·1	38·6	15·3
Year 1886.	83·5	54·90	21·7	43·17	49·03	15·3
Year 1885.	87·8	53·98	22·1	42·53	48·09	20·1
Year 1884.	87·5	57·44	22·6	44·07	50·66	23·7
Year 1883.	82·5	54·54	20·9	42·88	48·71	19·3
Year 1882.	78·5	55·46	21·9	43·62	49·54	20·6
Year 1881.	86·9	55·44	12·3	42·92	49·18	5·8

MONTH.	Number of Days on which the Minimum Ground Temperature was below 32°.	Number of Days on which the Minimum Air Temperature was below 32°.	Number of Days on which the Maximum Air Temperature was below 32°.	Number of Days on which the Mean Air Temperature was below 32°.
January . .	23	16	0	6
February . .	23	18	0	5
March . . .	18	16	0	5
April . . .	5	2	0	0
May . . .	1	0	0	0
June . . .	0	0	0	0
July . . .	0	0	0	0
August . .	0	0	0	0
September .	0	0	0	0
October . .	1	0	0	0
November .	6	0	0	0
December .	25	12	1	6
Year 1886 .	102	64	1	22

Year 1885 .	68	40	1	6
Year 1884 .	51	19	0	1
Year 1883 .	79	40	0	6
Year 1882 .	63	26	2	7
Year 1881 .	94	60	11	24

The tables show that the year 1886 was an average one for temperature on the whole. The month of February was remarkably cold, and January, March, April, August and December were colder than the average; the remaining months warmer, the three autumn months rather strikingly so.

The number of frosts in 1886 was much above the average, only 1881 at all compares with it.

MEAN SHADE TEMPERATURES OF THE MONTHS.

	1881.	1882.	1883.	1884.	1885.	1886.	Mean of Six Years.
January . .	31·8	40·7	42·3	44·2	38·7	35·5	38·87
February .	39·3	42·6	43·3	42·3	44·0	35·8	41·22
March . .	43·0	45·7	37·0	44·9	41·7	40·1	42·07
April . . .	47·3	48·8	47·5	45·1	46·7	46·4	46·97
May . . .	54·4	53·7	49·9	53·3	47·0	52·8	51·85
June . . .	56·9	56·0	57·2	59·8	58·4	58·8	57·85
July . . .	65·9	59·6	57·2	60·7	62·6	62·7	61·45
August . .	58·8	60·2	60·6	64·5	57·5	59·1	60·12
September .	56·6	53·9	55·3	59·4	54·5	58·2	56·32
October . .	46·5	50·2	49·6	48·9	45·6	52·9	48·95
November .	48·3	43·7	42·8	41·6	43·3	47·1	44·47
December .	40·8	39·8	41·4	43·1	38·8	38·6	40·42
The Year .	49·2	49·5	48·7	50·7	48·1	49·0	49·20

On the Origin of Mountain Ranges.

BY PROF. C. LLOYD MORGAN.

Read March 9th, 1887.

SITUATED as we are in Bristol on the folded Palæozoics, within sight of the Mendip anticlinal, and almost within sight of the denuded base of what was once a considerable mountain system in Wales, we cannot fail to be specially interested in any theory which endeavours to elucidate the process of mountain building and strata folding. Having moreover at our very doors the Clifton fault, within easy distance the Clapton-in-Gordano fault, and in our coal basins faults without number, we are led to take all the more interest in such a theory as attempts definitely to connect normal and reversed faulting with other great earth movements. I have therefore thought it well to bring before the members of the Geological Section of the Bristol Naturalists' Society some account of a hypothesis recently advocated by Mr. Mellard Reade, C.E., F.G.S., etc., in a volume bearing the same title as this paper, and to add thereto some few criticisms, together with some general remarks on this interesting but most difficult subject.

The hypothesis that has until recently found most favour in the eyes of geologists, is that according to which mountain ranges are regarded as the wrinkles on the earth's surface due to contraction consequent upon the fall of temperature which results from the constant radiation of earth-heat into space. Many facts of mountain structure, the extreme plication of the strata, the fan-structure, and the linear compression; and many facts of mountain distribution, the subsidiary spurs which accompany great ranges, and the complementary arrangement of the Eurasian systems, east and west, and the American, north and south, have been held to be readily explicable on this hypothesis.

But in 1881 the publication of the *Physics of the Earth's Crust* by Mr. Osmond Fisher, once a firm supporter of contraction, but herein an advocate of other views, dealt it a heavy blow, all the weightier from its (to most geologists) incomprehensible mathematics. Captain Dutton and other American geologists, well acquainted with all the details of mountain structure in the great western continent, have weighed it in the balance and pronounced it wanting. In the antipodes Captain Hutton is an old opponent with a rival theory of his own. And now Mr. Mellard Reade enters the field and contends that, not to contraction, but to expansion within the crust of the earth, are we to attribute the birth of a mountain range.

In any consideration of mountain-building a preliminary problem has to be dealt with. What is the condition of the interior of the earth? The old view of a molten globe surrounded by a thin solid crust is now held only by the few who are content to ignore, or who dare dispute, the results of physical and mathematical investigation. Geologists must choose between the two views which, we are confidently told, are alone physically possible: that of complete and thorough solidity, and that of a solid core and solid crust, with a stratum of molten rock between. Mr. Osmond Fisher advocates the latter view, Mr. Mellard Reade accepts the former. All agree, however, that the interior of the earth is intensely hot; all agree that this heat is being gradually but slowly lost by radiation; all agree that the result of such loss of earth heat is the contraction of the earth's volume. But whereas the contractionists contend that this earth-shrinkage is sufficient in amount to produce all those wrinkles on the face of mother earth which we little mortals call mountain ranges, both Mr. Fisher and Mr. Reade contest the sufficiency of this explanation.

Sir William Thomson has established—that is, calculated on the assumed correctness of certain data—the law which the temperature within the earth must follow if it be a solid cooling by conduction. Accepting this law, and making use of the results of experiment on the amount of contraction the rocks undergo on cooling, Mr. Fisher has calculated the amount of inequalities of the earth's surface which would have been formed had they been due to a hot solid globe cooling by conduction, and finds that the amount falls far short of that which is found in the inequalities actually existent. For whereas the actual inequalities, if levelled down, would form a layer of about 10,000 feet thickness, those which would be formed on the present hypothesis would form a layer certainly not exceeding 900 feet in thickness, and probably not exceeding 200 feet in thickness. (*Physics of the Earth's Crust*, p. 274.)

Mr. Mellard Reade rejects the contraction hypothesis on other grounds. He thinks "that an important link in the chain of reasoning has been omitted, inasmuch as the shrinkage of the solid shell itself through the gradual cooling of its lower zones has been unconsidered in the application of the hypothesis." He draws attention to Mr. Fisher's calculation that if the temperature of solidification were 7,000° Fah., our globe would have radially contracted from the time of solidification to the present time 1·9 miles, and contends that, owing to circumferential shrinkage, with a radial contraction of 2·04 miles, the crust at a depth of one mile would undergo no compression.

Both these authors, therefore, reject the contraction hypothesis on physical grounds, and contend that to produce the known inequalities of the earth's surface the contraction along a radius from the earth's centre, instead of being only 1·9 miles, ought to be from 30 to 40 miles.

But if the compression which is an observed fact on the earth's surface has not been produced by secular contraction, it must be due to expansion of the underlayers of the earth's crust. Both authors, therefore, base their hypotheses, in part at least, on such expansion. But they account for this expansion in different ways.

Mr. Fisher's hypothesis requires a liquid inter-stratum between the solid crust and the solid core. He suggests that fissures may be formed in the lower part of the crust, and may be injected with liquid from the fluid inter-stratum. The upper part of these fissures would be filled with gaseous water-substance at a pressure of 10,066 tons to the square foot. The enormous pressure of this gas would tend to widen the fissure and compress the rocks on either side of it. "When the rent reached the surface, the vapour would rush forth and be followed by the magma itself, now appearing as lava; and thus a volcano would be established. But this would be an exceptional occurrence. It would only be here and there that the vapour would escape at the surface, because its doing so at one point would relieve the internal pressure for a long distance." "The quantity of igneous rock injected into the fissures would go to increase the solid crust in the disturbed regions," and thus upper ends of the injected fissures would be recognised by geologists as dykes of igneous rock.

But the formation of a mountain ridge by such compression in a solid crust resting on a liquid inter-stratum would, by the laws of hydrostatic equilibrium, involve certain consequences. First, the thickening of the crust would not only take place upward, so as to give rise to the visible mountain mass, but also downwards, so as to give rise to solid roots of the mountains projecting into the liquid inter-stratum. Mr. Fisher calculates that two-fifths of the thickening would appear as a mountain chain, while three-fifths would go to form invisible mountain roots. "The existence of these roots of the mountains is not a mere matter of speculation. They have been felt by the plumb-line in the following manner. The great mass of the Himalaya mountains was, during the Indian Trigonometrical Survey, found to attract the plumb-line. But upon its being calculated how much attraction ought to be attributed to this mountainous mass, it was found that, though they attracted the plumb-line, yet they ought to have attracted it more than they did. Sir G. B. Airy explained this anomaly by the existence of downward protuberances of a lighter crust into a heavier substratum; this is exactly the same supposition to which our reasoning has led us." "Again, the existence of roots of the mountains ought to be revealed by phenomena of underground temperature; and we find such to be the case." A thickened crust in mountain regions ought to be accompanied by a diminished increment of temperature for a given increment of depth. During the construction of the St. Gothard and Mont Cenis tunnels, the increment was only about half the average; namely, 1° Fah. for 100 feet, instead of 1° Fah. for from 50 to 60 feet.

Secondly, "the thickened area would sag downwards, and the most thickened area would sink the most. Hence depressions would arise on both sides of the ridge, and the ocean, which covers the general surface, would be deeper than elsewhere along two channels parallel to, and at some little distance from, the ridge. But should the ridge be steeper on one side than on the other, as seems inevitable, the ocean would be deeper on the steeper side." This seems in accordance with fact. As Dana puts it, "the highest mountain-border faces the largest ocean."

Lastly, on the principles of hydrostatic equilibrium, areas of deposit, being constantly loaded with fresh sediment, will sink; while areas of denudation, being continually lightened of their load, will tend to rise. And differential denudation on opposite sides of a mountain axis will give the mountains a tendency to tilt.

Such is Mr. Fisher's hypothesis. I pass now to Mr. Mellard Reade's, merely interpolating an exclamation of regretful wonder that the newer writer should not have introduced into his volume a more detailed notice and criticism of the views of his predecessor in this interesting field of research.

Professor James Hall (1857) was apparently the first to give prominence to the fact that the preliminary and preparatory stage in mountain-building is the long-continued accumulation of great thicknesses of sedimentary deposits. Thus, whereas the total thickness of the Palæozoic strata in the Appalachian chain is about 40,000 feet, the same formation in the Mississippi Valley, including the carboniferous limestone, which is wanting in the east, attains a thickness of scarcely 4,000 feet. At an earlier date (1834) Babbage had shown that the deposition of fresh layers of sediment is necessarily accompanied by a rise of the isogeotherms, or planes of equal temperature beneath the earth's surface.

Given that the rate of increase of temperature as we sink into the earth's crust is 1° Fah. for every sixty feet, and that the average temperature of the surface is 50° Fah., then a pebble or shell at a depth of 600 feet will have a temperature of $50° + 10°$, or 60° Fah. Now, suppose 600 feet of sedimentary strata to accumulate over this spot; the pebble or shell will now have a temperature of $50° + 20°$, or 70° Fah. It has increased 10° in temperature for the 600 feet of sedimentation. Thus, accompanying the accumulation of great thickness of sedimentary deposits, there is a rise in the temperature of their lower layers. This rise of the isogeotherms is the starting-point of Mr. Reade's hypothesis.

For as surely as solids contract on loss of heat, so surely do they expand as their temperature is raised. From experiments of his own and of others, Mr. Reade takes 2·75 feet per mile as the average expansion increment of rock for every 100° Fah. Lyell had drawn attention to the *vertical* uplift due to such expansion. Captain Hutton introduced linear *horizontal* expansion as the main factor in mountain building. But "no one," says Mr. Reade, "so far as I can discover, seems to have perceived in connection with the earth's crust that increase of *volume* is the final result of expansion. Yet this must be the most potent of all in its geological effects."

"Let us now consider," he says, "what would be the quantitative effect towards the ridging up of a mountain range of a rise of temperature in a given area and depth of rock. Take a volume, for example, equal to $500 \times 500 \times 20$ miles; that is, 5,000,000 cubic miles. If this were heated to a mean of 1,000° Fah., a temperature that must have occurred over and over again in the local heating of the earth's crust, there would be a linear expansion in two directions of 13,750 feet, or 2·6 miles.

"It is evident, on very little consideration, that this mass of rock cannot expand laterally, for in that case it would displace the crust of the earth surrounding the affected area; nor downwards, for that would displace the solid foundations of the earth itself. It is only free to expand upwards. We may therefore take it that the lateral increase of volume will be transformed into an upward one. The lower strata would expand the most, the surface would not expand at all. The behaviour of the crust under these conditions would depend on the nature of the strata affected. If the beds were comparatively thin, they would become folded and packed together in a more or less vertical position. The internal strain would have most effect in the region of least expansion, and the packing of the strata in this locus would ridge up the surface and burst it along the lines of greatest weakness, for the surface layers would be unaffected by the subterranean heat."

Mr. Reade contends that under the intense pressures thus generated, even the hardest rocks, such as consolidated granites, whinstones, and sandstones, would act as plastic materials. "If the rocks forming the cores of such ranges as the Alps were crystalline before the upheaval of the mountains, it is quite evident that, hard and rigid as they were, they must have responded to the subterranean pressures, and *flowed* like lead through a die. In no other way does it seem possible to account for the forms these crystalline rocks have assumed."

The uplift on this hypothesis throws the superficial rocks into a state of tension. Hence great superficial cracks. It is from this cause, Mr. Reade thinks, that we find valleys of denudation situated more frequently on anticlinals than synclinals. Thus scarped faces are initiated. And he goes so far as to say, that except in those cases in which a scarped face originates in a fault, this is the only way in which a true escarpment can arise.

Thus to expansion of the underlayers do Mr. Reade's mountains owe their birth. But when the energies finally die out, contraction occurs on a large scale, and thus are produced those normal faults which intersect or run parallel with mountain axes. It might be thought that subsequent contraction would neutralize the effects of previous expansion, and that the mountain range would sink down once more into the bosom of the continent. But Mr. Reade's experiments on sheets of lead show "that that metal, even in small pieces, cannot pull itself back to its original shape on cooling. How much less must this be the case with rock!" "Every fall of temperature produces a proportionate vertical subsidence of the surface over the district affected; but as the materials laterally ridged up in mountain ranges by expansion cannot be drawn back again during contraction, there remains a permanent total of uplift in the range with every rise of temperature, that can only be removed by atmospheric denudation."

I have now given, I think, a fair though necessarily condensed account of Mr. Reade's hypothesis. I pass therefore from the expository to the critical part of this paper.

We have seen that the author says that increase of *volume*, as the final result of expansion (1), has hitherto remained unnoticed, and (2), must be the most potent of all in its geological results. But (1), this cubical expansion with consequent deformation was clearly described fourteen years ago by Mr. Fisher, in a paper in the *Geological Magazine* for June, 1873, to which Mr. Reade refers in his text. Mr. Fisher says: "Now, fixing our thoughts upon a cube of one foot of rock, it certainly seems probable that, under great horizontal pressure, it would undergo the small amount of compression involved and not expand at all horizontally, but simply become about one-hundredth of an inch higher. Or else the *whole* expansion would take place in the vertical, the block suffering slight *deformation*, and becoming about three-hundredths of an inch higher than it was originally." "As a homely instance of the effect I suppose, may be taken the case of a loaf of light bread baked in a 'tin.'" (*Geol. Mag.*, vol. x. p. 251). Mr. Reade is thus not the first to draw attention to this effect. Nor (2) when he comes to work out the results of expansion, does he take the expansion of *volume* as "the most potent of all in its geological effects." For in dealing with the effective increase in cubic miles of a given heated area, he *disregards vertical expansion*, telling us in a note that "it does not contribute to horizontal displacement and *consequent permanent ridging up*." He thus lays a good deal of stress on volume expansion on one page only to abandon it on the next.

That however is a minor point. A much more serious criticism is this. In his enthusiasm for his expansion hypothesis, he practically neglects to consider whether the superinduced metamorphism in the heated rock may not lessen or even neutralise the effects of mere expansion by heat. What are the views of others on this head? Dr. Sterry Hunt, no mean authority on such a question, says: "The effects of heat and water upon the buried sediments would be *condensation*, from the diminution of porosity, and still more from the conversion of the earthy materials into crystalline species of higher specific gravity, thus causing *contraction* of the mass" (quoted in *Physics of the Earth's Crust*, p. 202). And Mr. Osmond Fisher inclines to the same view; for in a criticism on Captain Hutton's theory, of which Mr. Mellard Reade's is in part a revival, he says: "But I would go farther, and inquire whether the internal heat of the earth would not produce contraction rather than expansion; at least if it was sufficiently great to introduce metamorphism or any chemical change." (*Geol. Mag.*, vol. x. p. 261). This is the view to which I believe most physical geologists will incline, and that the more since it seems to accord with the gradual sinking which accompanies continuous depression. I cannot but feel surprise that Mr. Reade should pass over this aspect of the question in almost complete silence.

And if the expansion does take place, with the rise of the isogeotherms, as is assumed by Mr. Reade, how is it that the vast masses of preparatory sediment (40,000 feet or more in some cases) continue to be formed *in a sinking area?* Why is the effect of expansion, which one would expect to take place *pari passu* with sedimentation, delayed for long geological ages? Does Mr. Reade answer that the rise of the isogeotherms is a slow and gradual process? So is sedimentation. A further question suggests itself. Why, when expansion does begin to take effect, is mountain upheaval so rapid? I do not mean rapid as historians count time, but rapid as geologists count time. Mr. Reade again and again reminds us that mountain building is a slow process; and so it is in one sense. But whereas the preparatory sedimentation for the Alps occupied all the Mesozoic and Eocene time, the first upheaval of that range can be dated distinctly as Oligocene. Compared with the ages of previous sedimentation, the upheaval of the Alps may be said to be sudden.

Let us now look at Mr. Reade's figures. He tells us that if we assume that in one area of the globe the temperature plane of 3,000° Fah. rises from a depth of thirty-five miles to a depth of twenty-five miles, the mean increase of temperature for the whole overlying twenty-five miles of rock would be 428°5. This assumption is surely bold enough. And yet in illustrating the results of such expansion on a later page, a mean temperature of more than twice this amount (1,000° Fah.) is assumed. "If this mass of rock were heated to a mean of 1,000° Fah.," he says, "a temperature that must have occurred over and over again in the local heating of the earth's crust," the results already described would follow. Here the words "increment of" should be inserted before 1,000° Fah., and then we shall perhaps hesitate before admitting that it must have occurred over and over again. If we stick to facts of observation, we have, say, 40,000 feet of continuous deposit. At an increment of 1° Fah. for every 60 feet of descent (assumed to be constant throughout this depth), this will give us 666°6̇ Fah., for the bottom layer, or a mean of 333°3̇ Fah. for the whole mass. And, as Mr. Fisher has pointed out, these 333°3̇ Fah. have been abstracted from the underlying layers of rock which are thereby temporarily cooled to that extent. I cannot but think an increment of 1,000° Fah. is, as they say in America, a "little too steep."

Still, let us accept it for the nonce; and let us suppose that all the expansion is effective, no part of it being neutralised by contraction due to metamorphism; even then we obtain only, on Mr. Reade's showing, a linear expansion in two directions of 2·6 miles. But in the Alps Prof. Heim estimates that the compression has been to the extent of seventy-two miles; and Prof. Claypole estimates that in the Appalachian chain, 153 miles have been compressed into sixty-five miles. Mr. Reade thinks such estimates excessive. They take no account, or not sufficient account, he thinks, of the squeezing out of the strata where they happen to lie parallel to the advancing plane of pressure, and assume that the tops of the anticlinals were continuous and not cracked, as he contends. No such objection can however be taken to the fact that at Durness in the Highlands certain strata have been pushed forward bodily over younger rocks for a distance of not less than ten miles. But even granting that his objections to the figures given by Professors Heim and Claypole have some weight, the discrepance, though it may be somewhat reduced, cannot possibly in this way be annihilated.

A further criticism is suggested by the consideration of the observed contortions in mountain ranges. Mr. Mellard Reade tells us that "the upper layers of the earth's crust being less affected by these variations in temperature as the surface is neared, are by the ridging up thrown into a state of tension"; whence arise, as we have seen, gaping cracks along the summits of the anticlinals. How comes it then, we may ask, that the Eocene strata, the latest of the long series of continuous deposits in the Alps, are so wildly contorted in those mountains? In our own district the main foldings of the strata took place, I believe, shortly after carboniferous times; but the fact that the then recently formed rocks are so folded and ridged is evidence that they were not thrown into a state of tension, but one of compression.

Another fact does not seem to have been duly weighed by Mr. Reade. If we call into play expansion by the rise of the isogeotherms due to sedimentation, must we not, by parity of reasoning, call into play contraction by the lowering of the isogeotherms due to denudation? The denudation of the Alps has been enormous. Must there not have been a proportional lowering of the isogeotherms, and therefore a proportional contraction? But where is the evidence thereof? In arguing against a rise of the land, proportional to the lightening of the burden by subaerial denudation, Mr. Reade says, "It may be pointed out that the removal of 30,000 feet of rock from the Uinta Mountains has not been followed by an equivalent uplift, if referred to the sea-level." Has it, I would ask, been followed by subsidence proportional to the consequent lowering of the isogeotherms?

The more I consider what must be the effects of subsequent contraction, the more difficult do I find it to accept Mr. Mellard Reade's hypothesis. Relying partly on his experiments on a small scale with lead, he tells us that the materials laterally ridged up by expansion cannot be drawn back again during contraction. Hence there remains a permanent total uplift in the range. "None of the material of the earth laterally displaced by expansion can be drawn back by tension, as is the case with metal plates; consequently the elevation," he adds, "is more rapid and effective." But the subsequent contraction *must* take place *somewhere.* I am ready to admit, and have indeed for many years been in the habit of teaching, that whereas reversed faults are due to intense lateral pressure, normal faults are the result of local and comparatively superficial contraction, and involve a lengthening of the area in which they occur. But I do not think that in any known area of the earth's surface there is evidence of so great an amount of local contraction as is necessitated by the after-tensions involved in Mr. Mellard Reade's hypothesis.

For many reasons therefore I feel constrained to reject Mr. Mellard Reade's views, gladly as I welcome his volume, as presenting an adequate account of the phenomena of mountain building; and as a minor criticism, I would question his view of the plasticity of solid granite or gneiss. The analogy of the flow of metals will not help us here. I rather incline to the view that the gneiss, which exhibits in the Alps the well-known fan structure, was, during the throes of mountain upheaval, in an imperfectly solid condition through incipient aqueo-igneous fusion.

I am not here to-night to advocate any theory of my own. I am inclined to believe that, after all that has been said against it, the hypothesis of secular contraction has, mathematics notwithstanding, some vitality in it yet. The view that long-continued sedimentation gives rise to a line of weakness (partly through rise of the isogeotherms) between the continental area, strengthened perhaps by a depression of the thermal planes through denudation, and the oceanic area, strengthened perhaps by a depression of the isogeotherms through convection, and that along this line of weakness a mountain range is ridged up through secular contraction, still seems to me, if not the most probable, at any rate the least improbable hypothesis.

I do not question for a moment the accurate working of the mathematical mill which has produced Sir William Thomson's and Mr. Osmond Fisher's results. But I think the data may need revision. And the accuracy of the result depends entirely upon the validity of the data. Sir William Thomson's results are based, it seems, upon the cooling of (1) a solid globe, (2) by conduction. But accepting for the nonce Mr. Fisher's interstratum of molten rock, which we may presume has been gradually lessening in amount by gradual solidification: has sufficient account been taken of the consequent contraction on solidification to granite or other such rock, estimated by M. Delesse at from three to seven *per cent.?* has sufficient account been taken of the removal from the underlayers of vast amounts of rock through the instrumentality of volcanic action? and has sufficient account been taken of the large amount of heat-energy removed by the underground circulation of water, and by the vast volumes of steam formed during volcanic eruptions? The annual discharge of the Bath spring alone is more than sixty-five million gallons at a temperature of about 120° Fah. I should like to see a calculation of the number of units of heat lost to the under-strata annually through the instrumentality of hot springs. I believe it to be one of the functions, and not the least important functions of water to be constantly diving into the earth cold and rising to the surface in a more or less heated condition. I am not aware whether this has been ever sufficiently taken into consideration. It seems to me to be a highly important agency by which the underlayers of the crust are made to cool and contract more rapidly than the overlayers. It may help too to account for the diminished temperature beneath mountain ranges; the diminished density beneath continental as compared with oceanic areas being explicable on that hypothesis of differential cooling along different radii which accounts for the initial determination of these oceanic and continental areas.

In conclusion, I would beg to draw the attention of the members of this section to the many valuable facts and illustrations of mountain structure which Mr. Mellard Reade has collected together in his volume. I do not think his theory will be generally accepted. But then are we not all of us much in the dark on this difficult question? Might not some good-humoured cynic liken our attempted solutions to the sweet twitter of the sea-lark in Browning's song, mountain ranges being but part of the "good gigantic smile of the brown old earth"?

"Oh, good gigantic smile of the brown old earth,
  This autumn morning! How he sets his bones
To bask in the sun, and thrusts out knees and feet
For the ripple to run over in its mirth:
  Listening the while, where on the heap of stones
The white breast of the sea-lark twitters sweet!"

The Potato Tercentenary.

BY GEORGE F. BURDER, M.D., F.R. MET. SOC.

PROMINENT among the features of the present day are jubilees, centenaries, tercentenaries, quincentenaries, even millenaries; and celebrations of this kind, if only the subject be worthy, are to be commended as supplying a special motive for the study of historical events in their bearings upon the progress of our race.

The introduction of the potato into Britain was an event of much more than botanical interest, and it was a happy thought to signalise what was believed to be its three hundredth anniversary by a conference and exhibition in London, at which should be gathered together all the available means for ascertaining the time and circumstances of the event, as well as for discussing matters bearing upon the future of this valuable root.

The conference was held in the early part of December, 1886, and the exhibition is said to have been remarkably rich in materials for illustrating the history of the potato. Nevertheless, the result, so far as relates to the introduction of the plant into this country, is disappointing to those who had not the advantage of being present, nothing, to the best of my knowledge, having been published beyond short abstracts of the proceedings, and very little indication given of the grounds upon which the speakers based their conclusions.

Of the various subjects relating to the potato which were discussed at the conference, the one which I propose to take up is the introduction of the plant into Britain, and its early history here.

We know a great deal more of the history of the potato than we can ever hope to know of the history of wheat or other cereals; the history of these latter being lost in the mists of a remote antiquity, while that of the potato, considered as a European plant, is comparatively modern. No botanist can point with certainty to the wild plant from which our cultivated wheat has been developed, or name its native habitat. The wild potato, on the other hand, and its habitat, are matters upon which botanists are, to a large extent, agreed. Still there are many points regarding the history of the potato which admit of controversy, and some, probably, which will never be settled.

Three persons have been credited by different writers with the introduction of the potato into England—Sir John Hawkins, Sir Francis Drake, and Sir Walter Raleigh—all, curiously enough, Devonshire men, and all prominent figures in the early history of the British navy.

The earliest date assigned in books to the introduction of the potato into England is 1563. In "Haydn's Dictionary of Dates," I read—"Potatoes were originally brought to England from Santa Fé, in America, by Sir John Hawkins, A.D. 1563"; and a similar announcement may be found in other books of reference. Hawkins, to whom belongs the unenviable distinction of being the first Englishman to engage in the African slave-trade, made three voyages about the above-mentioned date for the purpose of supplying the Spanish colonies in the West Indies with slaves from Guinea. The first of these voyages was begun in 1562, and ended in 1563; the second was accomplished in 1564–5; and the third, commenced in 1567, was finished, as the narrator says, in January, 1568, or as we should say, in January, 1569, the new year at that date being considered to begin in March. If Sir John Hawkins brought home potatoes in 1563, as alleged in the books, it must have been on his return from his first voyage; but that is rendered improbable by the fact that his first voyage included no visit to the mainland of America. In Hakluyt's collection there are narratives of all three of Hawkins's voyages, and a perusal of the second of these discovers plainly the origin of the statement referred to, which, at all events, will be seen to involve an error of date. I extract the following from the account of Hawkins's second voyage in the black-letter edition of Hakluyt, vol. iii., p. 507:—

> "Here perceiving no trafficke to be had with them, nor yet water for the refreshing of our men, we were driven to depart the twentieth day, and the 2 and twentieth we came to a place in the maine called Cumana, whither the Captaine going in his Pinnisse, spake with certaine Spaniards, of whom he demanded trafficke, but they made him answere, they were but souldiers newely come thither, and were not able to buy on [? one] Negro: whereupon hee asked for a watring place, and they pointed him a place two leagues off, called Santa Fè,* where we found marveilous goodly watering, and commodious for the taking in thereof: for that the fresh water came into the Sea, and so our shippes had aboord the shore twentie fathome water. Neere about this place, inhabited certaine Indians, who the next day after we came thither, came down to us, presenting mill and cakes of breade, which they had made of a kinde of corne called Maiz, in bignesse of a pease, the eare whereof is much like to a teasell, but a spanne in length, having thereon a number of granes. Also they brought down to us Hennes, Potatoes and Pines, which we bought for beades, pewter whistles, glasses, knives, and other trifles.
>
> "These Potatoes be the most delicate rootes that may be eaten, and doe farre exceed our passeneps or carets."

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* Santa Fé, near Cumana, in Venezuela.

This passage seems at first sight conclusive as to the fact of Hawkins having at least seen potatoes, whether he brought any home or not; but, for reasons which will appear presently, it is questionable whether the roots which he calls potatoes were potatoes in our sense of the term.

The evidence in favour of Sir Francis Drake having introduced the potato into England in 1586 is considered by Mr. Mitchell, one of the speakers at the conference, as sufficiently conclusive. It was in that year that Drake brought back from Virginia the colonists whom Raleigh had established there in the previous year, and who had been unable to hold their ground against the hostility of the Indians.

The association of Sir Walter Raleigh's name with the introduction of the potato appears to rest on no better foundation than the two following facts—one assumed, the other established: first, that it was in connection with the return of Raleigh's colonists from Virginia that the introduction took place; and, secondly, that Raleigh himself, if he was not the first to introduce the plant into Ireland, at all events cultivated it successfully in his garden at Youghal.

Assuming what seems extremely probable, that the same ship which brought back the Virginian colonists brought also the first potatoes to this country, the credit of the introduction may, perhaps, fairly be divided. To Raleigh a portion may be assigned, as the organiser of the Virginian settlement, and to Drake another portion, as the commander of the ship. But, as it seems to me, a good third of the credit should be given to a man whose name has seldom been heard in the controversy, one Thomas Harriot.

Thomas Harriot, who was an eminent mathematician and astronomer, formed one of the party of colonists sent out by Raleigh to Virginia, and he has left on record an interesting account of the productions of that country. I quote what he says of a certain root which he found, and which there seems very good reason for identifying with our own potato:—

> "Openauk are a kinde of roots of round forme, some of the bignesse of Walnuts, some farre greater, which are found in moist and marish grounds growing many together one by another in ropes, as though they were fastened with a string. Being boiled or sodden, they are very good meat."—*Hakluyt, Black-Letter Ed.*, vol. iii., p. 272.

There are many other descriptions of natural products; and, in short, Harriot seems to have been, as we should say in modern parlance, the naturalist of the expedition. He was in the country for twelve months, and had ample opportunities of observing. Under these circumstances it seems quite reasonable to assume, in the absence of positive evidence, that if any one of the company on board Drake's ship was more concerned than another in bringing over the potato, that one was Harriot.

It must be allowed that there is one serious difficulty in the view I have advocated, or in any view which assumes that the potato came from Virginia. The potato is not indigenous in Virginia, nor within a thousand miles of Virginia. Mr. Mitchell, at the conference, expressed his belief that Drake obtained the tubers in South America, where he had been before calling for the colonists in Virginia. But against that view must be placed the fact that these roots, on their introduction, and for many years afterwards, were always called Virginian potatoes, even by botanists—an error, if it was an error, which could scarcely have escaped correction when the facts were fresh in memory. Mr. Carruthers, at the conference, suggested, if I understand the report correctly, that the potato, though not indigenous in Virginia, might have spread thither by cultivation. There is said to be good evidence that the Indians of South

America had cultivated the plant from time immemorial, and Mr. Carruthers's suggestion is, perhaps, the most likely solution of the difficulty, notwithstanding that it seems to conflict with an express statement of Harriot, that the "openauk" was not a cultivated but a wild plant.

To appreciate aright the evidence on this point, it is necessary to inquire more particularly into the natural habitat of the potato and its allies. For this purpose I avail myself of a valuable article by Mr. J. G. Baker, of Kew, published in the Journal of the Linnean Society (*Botany*, vol. xx., No. 131).

The genus *Solanum*, to which the potato belongs, is one of the largest known to botanists. No less than about nine hundred species have been described, and it is estimated by Bentham and Hooker that some seven hundred of these are really distinct. Fortunately, however, for the inquiry in hand, only a very small proportion of these seven hundred species belong to the tuber-bearing group. About twenty tuber-bearing *Solanums* have been named, but Mr. Baker, in the article referred to (which was published in 1884), reduces these to six. These are the *Solanum tuberosum* (to which species belong, probably, all our cultivated varieties), the *S. Maglia*, the *S. Commersoni*, the *S. cardiophyllum*, the *S. Jamesii*, and the *S. oxycarpum*. By the report of the Potato Conference, I see that Mr. Baker now considers five kinds only to be specifically distinct, having merged the *Maglia* with the *tuberosum*. This is of interest, because the *S. Maglia*, the potato which Darwin found in the Chonos Archipelago, far south on the coast of Chili (lat. about 45°), is the kind which has lately been experimented upon by Messrs. Sutton, of Reading, at the instance of Earl Cathcart, with a view to establish, by cross-breeding, a kind which should be better adapted to the English, and, more especially, one may suppose, to the Irish, climate, than any of the existing varieties, and, therefore, stronger to resist disease. The expectation that such a result might ensue was founded upon the fact that while the *tuberosum* loves the dry and sterile regions of Central Chili, where no rain falls for six months, the *Maglia* luxuriates in the humid forests of the Chonos Islands.

According to Mr. Baker, the *S. tuberosum* inhabits the Andes of Chili, Peru, Bolivia, Ecuador, and Columbia; also the mountains of Costa Rica, Mexico, and the south-western United States; while the variety *Maglia* is found on the low land of the Chili shore, as far south as the Chonos Archipelago. The four other species of tuber-bearing *Solanums* are all likewise American, and are found in La Plata, Uruguay, Mexico, and the south-western United States, From this it seems clear that the "openauk" of Harriot, if really potatoes, could not have been, as his statement would appear to imply, indigenous plants, but must have been introduced by cultivation, having been derived in the first instance from a very distant region. It is possible, however, that the plant, though originally introduced, may in the course of time have become naturalized, and therefore in a legitimate sense wild.

The difficulties of this inquiry have been increased by the circumstance, not always recognised, that there were potatoes in England and on the continent of Europe before the discovery of the *Solanum tuberosum*. These *pre-solanal* potatoes were what we now call "sweet potatoes." They were the tubers of a convolvulaceous plant, the *Convolvulus batatas*, otherwise *Batatas edulis*. Whence and when they originally came, and to what extent they were used in this country, are questions more difficult to answer than any we have yet considered. The plants yielding them are found both in the

East and in the West—in China, Japan, Australia, th Pacific Islands, America, and the West Indies. They are cultivated in the southern countries of Europe, but they will scarcely grow in our climate. They were formerly imported from Spain, and are still occasionally seen in the shops. The name "batatas" is said (I know not on what authority) to be Malayan. If that be so, it may be taken as proving that they came originally from the East. The Spaniards may have obtained them either from the East or from the West, and their introduction into Europe may have been one of the early fruits of the Spanish voyages and conquests in the sixteenth century. Obviously our word "potato" is nothing but "batatas" modified. When the *Solanum* tuber was introduced, bearing as it did some resemblance to the potato then in use, and being cooked and eaten in the same way, it came to be called by the same name, with the word "Virginia" or "Virginian" added by way of distinction. Gerard, in his "Herbal" (1597), makes this very clear. He devotes a separate chapter to each plant, and each is illustrated with a woodcut. The chapter on the old-fashioned potato is entitled "Of Potatoes," and the drawing is headed—

"*Sisarum Peruvianum, sive Batata Hispanorum.*
"Potatus or Potatoes."

I extract the following from his description of the plant:—

"This plant which is called of some *Sisarum Peruvianum*, or Skyrrits of Peru, is generally of us called Potatus or Potatoes. It hath long rough flexible branches trailing upon the ground, like unto Pompions; whereupon are set rough hairie leaves, very like unto those of the wilde Cucumber. There is not any that hath written of this plant, or saide any thing of the flowers, therefore I refer the description thereof unto those that shall heereafter have further knowledge of the same:* yet have I had in my garden divers roots that have florished unto the firs approch of winter, and have growen unto a great length of branches, but they brought not foorth any flowers at all; whether bicause the winter caused them to perish before their time of flowring, or that they be of nature barren of flowers, I am not certaine. The rootes are many, thicke, and knobbie, like unto the rootes of Peionies, or rather of the white Asphodill, joined togither at the top into one head, in manner of the Skyrrit, which being divided into divers parts and planted, do make a great increase, especially if the greatest rootes be cut into divers gobbets, and planted in good and fertill ground.

"The Potatoes grow in India, Barbarie, Spaine, and other hotte regions, of which I planted divers rootes (that I bought at the exchange in London) in my garden, where they flourished until winter, at which time they perished and rotted."

Gerard's chapter on the potato then newly introduced is entitled, "Of Potatoes of Virginia," and the heading over the woodcut is—

"*Battata Virginiana sive Virginianorum, & Pappus.*
"Potatoes of Virginia."

Of this plant he writes:—

"Virginia Potatoes hath many hollowe flexible branches, trailing uppon the grounde, three square, uneven, knotted or kneed in sundry places at certaine distances; from the which knots commeth foorth one great leafe made of divers leaves, some smaller, & others greater, set togither upon a fat middle rib by couples; of a swart greene colour tending to rednes. The whole leafe resembling those of the Parsnep, in taste at the first like grasse, but afterward sharp and nipping the toong: from the bosome of which leaves come foorth long rounde slender footstalks, whereon do grow very faire & pleasant flowers, made of one entire whole leafe, which is folded or plaited in such strange sort, that it seemeth to be a flower made of sixe sundrie small leaves, which cannot easily be perceived, except the same be pulled open. The colour whereof it is hard to expresse. The whole flower is of a light purple color, stripped down the middle of every folde or welt, with a light shew of yellownes, as though purple and yellow were mixed togither: in the middle of the flower thrusteth foorth a thicke fat pointell, yellow as golde, with a small sharpe greene pricke or point in the middest thereof. The fruite succeedeth the flowers, round as a ball, of the bignes of a little bullesse or wilde Plum, greene at the first, and blacke when it is ripe; wherein is contained small white seede, lesser than that of Mustarde. The roote is thicke, fat, and tuberous; not much differing either in shape, colour or taste from the common Potatoes, saving that the rootes hereof are not so great nor long; some of them round as a ball, some ovall or egge fashion, some longer, and others shorter: which knobbie rootes are fastened unto the stalkes with an infinite number of threddie strings.

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* In Johnson's edition of Gerard (1633) this passage is given as follows:—"There is not any that have written of this plant have said anything of the floures: therefore I refer their description unto those that shall hereafter have further knowledge of the same." This seems to make better sense.

"It groweth naturally in America where it was first discovered, as reporteth *C. Clusius*, since which time I have received rootes hereof from Virginia, otherwise called Norembega, which growe and prosper in my garden, as in their owne native countrie.

"The leaves thrust foorth of the ground in the beginning of May: the flowers bud foorth in August. The fruit is ripe in September.

"The Indians do call this plant *Papus* (meaning the rootes) by which name also the common Potatoes are called in those Indian countries. We have the name proper unto it, mentioned in the title. Bicause it hath not onely the shape and proportion of Potatoes, but also the pleasant taste and vertues of the same, we may call it in English Potatoes of America, or Virginia.

"The temperature and vertues are referred unto the common Potatoes; being likewise a foode, as also a meate for pleasure, equall in goodnesse and wholesomnesse unto the same, being either rosted in the embers, or boiled and eaten with oile, vineger and pepper, or dressed any other way by the hand of some cunning in cookerie."

We are now in a position to re-consider the statement that Sir John Hawkins brought potatoes from Santa Fé.

On the one hand, it may be said that the very fact of Hawkins calling the tubers by the familiar name "potatoes" seems to show that the objects themselves were familiar to him, that is to say, that they were the tubers of the *batatas*, and not of the *solanum*. On the other hand, it may be contended that he was led to call them potatoes from their resemblance only to the root with which he was familiar under that name, and that his concluding remark about them is inconsistent with the supposition of their identity. Be that as it may, evidence is wanting of Hawkins having introduced the plant into England; while the terms in which Gerard, nearly thirty years later, writes of the Virginian potato as a new discovery, forbid us to believe that the plant had been long in cultivation in this country.

It was Bauhin, a Swiss botanist, who (about 1620) first referred the Virginian plant to the genus *Solanum*, calling it *Solanum tuberosum esculentum*—names (except *esculentum*) adopted by Linnæus and still retained.

Many years elapsed before the new potato so far supplanted the old as to be able to dispense with its distinctive epithet. So late as 1686—just a century after its introduction—Ray, in his "History of Plants," give as the English name "Virginia Potatoes," while he calls the other sort "Spanish Potatoes." It would seem as if at that time the two kinds had reached an equality of popularity, and the term, "potato," by itself, had become ambiguous. But there can be little doubt that for many years after the introduction of the Virginian root, the word "potato," used alone, retained its old meaning. It occurs twice in Shakespeare, in plays written respectively in 1602 and 1609, and in both instances there is a probability, from internal evidence, that the *batatas* tuber is meant.* Bacon, in his essay "On Life and Death" (about 1625), writes:—"If ale should be made not only with the grains of wheat, barley, oats, pease, and the like, but also should admit a part (suppose a third part to these grains) of some fat roots, such as are potado roots, pith of artichokes, burre roots, or some other sweet and esculent roots; we suppose it would be a more useful drink for long life than ale made of grains only." (*Montagu's ed. of Bacon's Works*, vol. xiv., p. 383.) This passage has been quoted as referring to the *solanum* potato, but considering the date and the absence of any distinctive addition, I should think it quite as likely that the sweet potato was intended.

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* The evidence is hardly conclusive. With regard to one of the passages (*Merry Wives of Windsor*, Act V., Sc. 5), it may be objected that the provocative quality fancifully ascribed to the batatas was quickly transferred, by the lively imagination of our forefathers, to the new root, as appears clearly from Gerard's description, as well as

At a little earlier date (1613), Queen Anne (wife of James I.) is said to have entered in her diary a purchase of potatoes at two shillings a pound, but without further particulars it would be impossible to say certainly which kind was meant.

With our knowledge, it is strange to see how very slowly the new vegetable crept into favour. In March, 1662–3, at a meeting of the Royal Society, a letter was read from Mr. Buckland, a Somerset gentleman, recommending the planting of potatoes in all parts of the kingdom, to prevent famine. Lancashire took the lead in the cultivation. In 1684 potatoes were first planted in open fields in that county, and in 1728 in Scotland. But it was not until about 1750 that potato fields had become at all general in

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from that given by Clusius, in his *Rariorum Plantarum Historia*, lib. iv., cap. 52 (1601).

With regard to the other passage (*Troilus and Cressida*, Act V., Sc. 2), the figure may admit of interpretation under either view.

It is even possible that the word "potato" may have been used by Shakespeare (perhaps also by Bacon in the quotation which follows) in a sense inclusive of both varieties, for it should be remembered that the botanical distinctions which now require us to consider the two roots as widely different products were then unknown.

the country. Even so late as 1760 (or thereabouts), Philip Miller, in his "Gardener's Dictionary," says of potatoes, that they were "despised by the rich, and deemed only proper food for the meaner sort of persons." But after this date they must have made rapid progress in public esteem, seeing that at the present time, according to official returns, the annual production of potatoes in the United Kingdom is about six millions of tons—a total representing upwards of three hundredweight for each individual, man, woman, and child.

The Senses and Sense-Organs of Insects.

By Prof. C. LLOYD MORGAN.

Abstract.

"NOTHING in natural history is more abstruse and difficult than an accurate description of the senses of animals." So wrote the Danish naturalist, Fabricius, nearly a hundred years ago. Recent advances in science have not proved his saying untrue. Even in animals constructed on the same morphological plan as man, there are probably great differences in the ratios of the senses. But where the animals are constructed on a different plan, the "abstruseness and difficulty" are enormously increased.

The special senses of man may be divided into two classes:

1. *Contact senses*, including,
   (*a*) *Touch*: by means of which we become immediately acquainted of the presence of matters solid, liquid, and gaseous.
   (*b*) *Taste*: where the matters are liquid or gaseous.
   c) *Smell*: where they are gaseous.

2. *Telæsthetic senses*, including,
   (*d*) *Hearing*: telling us of certain states of distant bodies through the intermediation of air-borne undulations ranging up to some 36,000 per second.
   (*e*) *The temperature sense*: telling us of certain states of distant bodies through the intermediation of ether-borne undulations of upwards of thirty million million per second.
   (*f*) *Sight*: when the ether-borne undulations range between about 400 and 800 million million per second.

The human organization is not fitted to respond to air-borne undulations beyond about 36,000 per second, and this upper limit of hearing varies considerably in different individuals. Nor does it respond to ether-borne undulations below about thirty million million per second. Below about 400 million million and beyond about 800 million million undulations per second the human eye perceives nothing, the end-organs of sight are not stimulated. But photography makes us indirectly acquainted with undulations up to 1,600 million million per second. This may be termed the *doctrine of limits* in sensation.

There would seem to be no organic reason why the sense-organization of insects should have the same limits as that of man. The probabilities are that the limits for them are different, perhaps very widely different. Sir John Lubbock has shown that ants are sensitive to ultra-violet rays. It is quite conceivable that the ants' nest may resound with insect voices inaudible to us, beyond our upper limit of hearing. Moreover, while we hear sounds of different pitch, and distinguish colours of different vibration-period, for us heat has neither tone nor colour. But it may be otherwise with insects. It is conceivable that the long summer's day, the warmth of which has for us neither tone nor colour, may to the butterfly be made musical with a symphony of solar radiance. I do not for one moment assert that this is so. My purpose is merely to show that there may be "permanent possibilities of sensation" to the insect of which we duller folk may only sometimes dream, with a sort of nightmare consciousness, the while, that folk yet duller still are laughing at us for our pains.

The speaker passed in review the more recent advances in our knowledge of the morphology and physiology of insect sense-organs. Concerning the psychology of the subject, he maintained that we could only make more or less improbable guesses from analogy.

He pointed out that the important *rôle* which sense-hairs or setæ play in organisms ensheathed in chitinous armour; and compared in this respect insects and their allies the crustacea. In Fig. 1, A B C D (from the writer's *Animal Biology*, by the kind permission of Messrs. Rivington) tactile, olfactory, and auditory setæ from the crayfish are shown.

The structure of the arthropod eye and the principle of arthropod vision was considered. Fig. 1, E shows a section of the eye of a crayfish, while F is intended diagramatically to illustrate the principle of mosaic vision. At *a b* are a number of transparent rods, separated by pigmented material absorbent of light. They represent the crystalline cones of E. At *c d* is an arrow placed in front of them. At *e f* is a screen placed behind them. Rays of light start in all directions, from any point *c* of the arrow; but of these only that which passes straight down one of the transparent rods reaches the screen. Those which pass obliquely into other rods, are absorbed by the pigmented material. Similarly, with rays starting from other points of the arrow. Only those which pass straight down one of the rods reach the screen. Whence there is thrown on the screen a reduced stippled image, *c′ d′*, of the arrow. On this view, therefore,

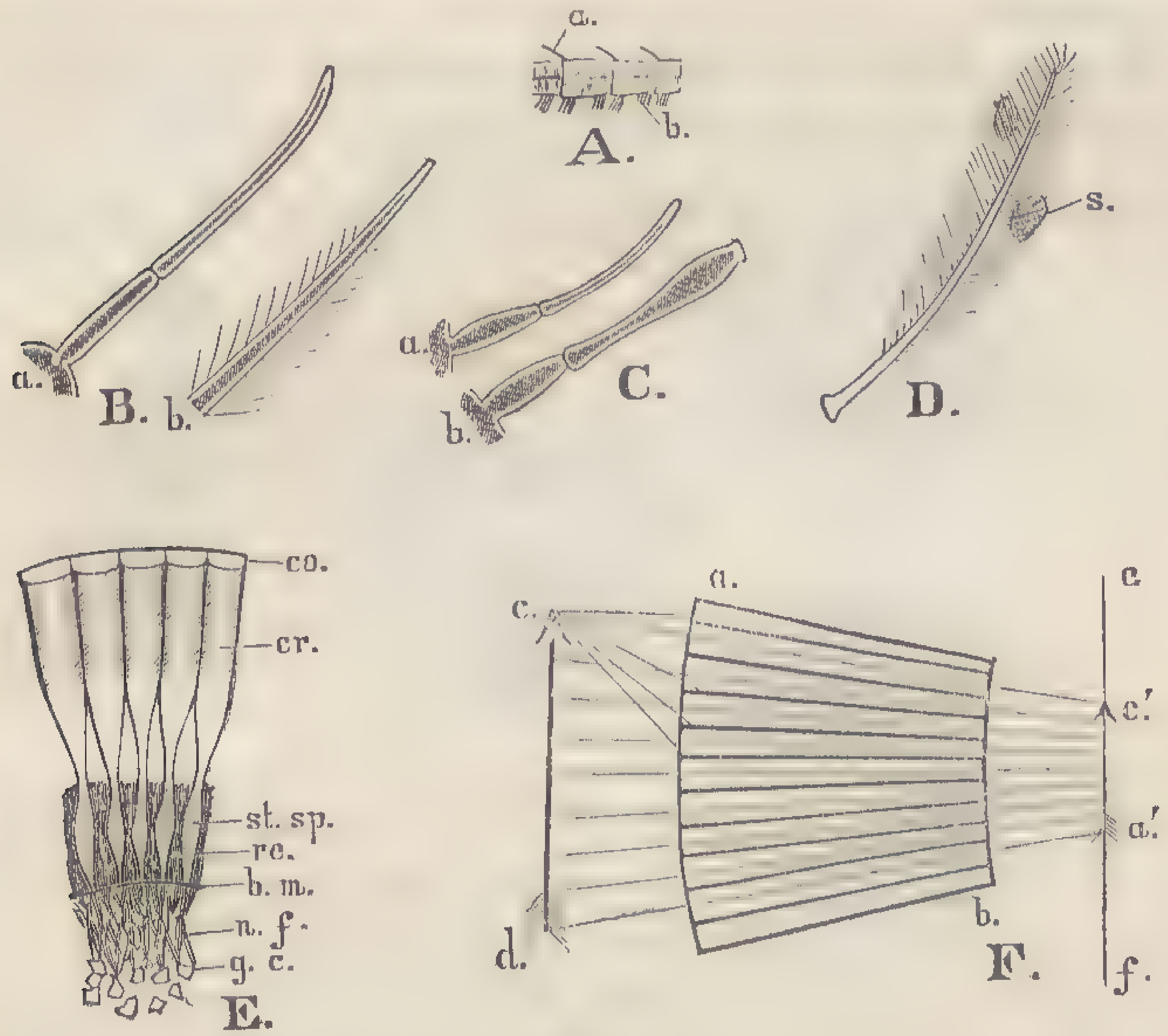

Fig. 1.

Sense-Organs of Crayfish.

(From C. Lloyd Morgan's *Animal Biology*. Fig. 78.)

A. Joints near the end of the outer limb (exopodite) of the antennule, showing tactile setæ at *a*, and olfactory setæ at *b*.

B. Tactile setæ—*a*. from the tip of the inner limb (endopodite) of the antennule; *b*. from a tuft on the antennulary protopodite on the base of the antennule. Drawn under the microscope.

C. Olfactory setæ—*a*. from the side; *b*. from above. Drawn under the microscope.

D. Auditory setæ from the auditory sac, which lies in the basal joint of the antennule. Drawn under the microscope. *s*. Grain of sand.

E. Portion of section of eye. *b. m.* Basilar membrane; *co.* corneal facet; *cr.* crystalline cone; *g.c.* ganglion cells; *n.f.* nerve fibres; *re.* retinula; *st. sp.* striated spindle. Drawn under the microscope.

F. Diagram to illustrate arthropod vision. *a.b.* Transparent rods with pigment between; *c.d.* object; *c′.d′.* its image on the screen *e. f.*

the so-called compound eye does not give a number of images of the object, but a single stippled image, or image in mosaic.

A few words were said, in conclusion, concerning the sensorium and nervous system in insects. The brain of the cockroach is shown in Fig. 2, from the writer's *Animal Biology*, by permission of Messrs Rivington.

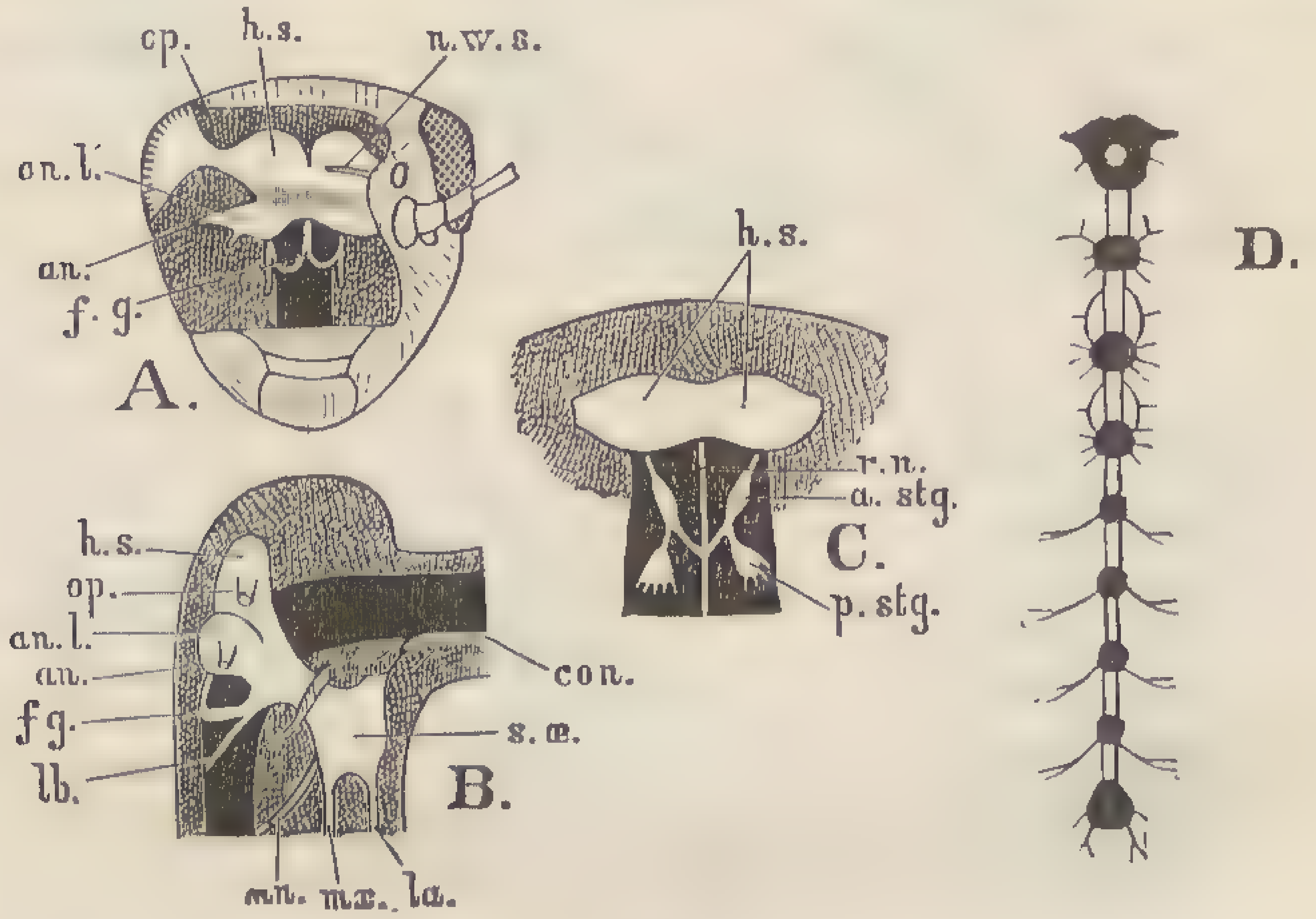

Fig. 2.

Brain and Nerve-Chain of Cockroach.

(From C. Lloyd Morgan's *Animal Biology*. Fig. 82.)

A. B. C. Head of cockroach opened so as to expose the brain: A. from the front; B. from the side; C. from above (after E. T. Newton). D. Nerve-chain, removed from the ventral region of the cockroach. *an.* Antennary nerve; *an. l.* antennary lobe; *a. stg.* anterior stomato-gastric ganglion; *con.* commissure to thoracic ganglia; *f.g.* frontal ganglion; *h. s.* hemisphere of brain; *la.* labial nerve; *lb.* nerve to labrum; *mn.* mandibular nerve; *mx.* maxillary nerve; *n. w. s.* nerve to white spot; *op.* optic nerve; *p. stg.* posterior stomato-gastric ganglion; *r.n.* recurrent nerve; *s. œ.* sub-œsophageal ganglion. The gullet is darkly shaded.

Note.—For a short popular account of the senses and sense-organs of insects, the reader is referred to an article on *The Honey Bee*, in the current (August) number of *Murray's Magazine*.

Note on a Sacred War Trophy from Ecuador,

CONSISTING OF A HUMAN SCALP AND FACE.

By EDWARD WILLIAM PHIBBS.

PROBABLY not more than three or four specimens of this exhibit have been seen in this country,—at least not more than three are recorded, two of which, however, are open to the suspicion of being identical,—a rarity which may be attributed to their being regarded as sacred, and therefore only obtainable under something like false pretence or cajolery.

The first public reference to one of these strange heads appears in a paper in "The Transactions of the Ethnological Society" of London, vol. ii. 1863, consisting of a translation of a document written by Don Ferro, Chilian Consul from Ecuador, by Mr. W. Bollaert, F.R.G.S., in which it is stated the natives of Ecuador after a war select the heads of their bravest victims, empty the skulls of their contents, and then by inserting a hot stone the skin becomes dessicated, and the whole equally reduced to miniature dimension. That, however, is only as he understood it, and he admits he may have misunderstood the process.

When the head is thus prepared, the tribe is called together before the expiry of nine days after the fight, otherwise the spirits of the killed Jívaros would not be satisfied, nor could the head be deified. On the tenth day a feast is held, and the natives, both men and women, dressed in holiday grandeur, surround the priest, while he in great state goes through the ceremony of making a holy oracle. After giving a minute description of this ceremony, which need not be here repeated, the writer adds: "It sometimes happens that, being hard pressed by the enemy, they have not time or opportunity to cut off the head of a fallen foe; in this case the ceremony is performed upon the head of a sow, which is adored as if it were an idol head." But as no such head has been seen or mentioned elsewhere, this statement may be only hearsay.

The specimen thus referred to was shown in the International Exhibition of 1862, in the Ecuador court, and was the second instance witnessed in England. We were told it was stolen from a small temple on the river Patassa, and that it was worn in battle as a charm. Professor Owen "thought it had been reduced by tanning the skin"; and Mr. Bollaert adds, "It struck me that it might have been shrunk and dried over a fire on a mould of clay." Others again have suggested slow drying by hot sand; but none of these processes can be accepted without hesitation, they being more or less inconsistent with our experience. Mr. Frank Buckland, the best taxidermist of his day, remarks on this matter: "I must confess that a great deal of dexterity and ingenious manipulation has been employed in a manner of which I am sorry to say I am at present ignorant."

I am indebted to Dr. Alfred Pullar for the loan of the specimen before us, which was sent to him by his relative residing near Ecuador. We may briefly describe it as a human head shrunk to about the size of a large orange without causing any distortion or disproportion of a feature; eyelids and mouth closed; hair about twenty-four inches long, intensely black, falling in graceful waves, and singularly beautiful in quality, hue, and brightness; skin very dark brown and hard, feeling like dried leather; a cord made of several cotton strands slightly twisted passes twice through the lips, tying them together, the ends then plaited hang down about eighteen inches; another cord is fastened to the top of the head for the purpose of suspension.

It is further stated that when this oracle has to be consulted, the priest first drugs the inquirer with some narcotic, and then, taking away the cords that seal the lips, places his ear to the mouth, and after listening repeats the answer imparted to him.

To canonize the head of an enemy intentionally killed is surely a strange freak in psychical phenomena!

Measured by comparison with their surrounding races, the Jívaros are a remarkable people. Velasco, the historian of Quito, speaks of them as brave, astute, warlike, muscular, and hospitable. Having a history of more than three centuries' duration, and having retained their independence whilst their neighbours were subjugated by the Spaniards, their strong individuality is obvious. Some of these independent characteristics may however owe something to the fact of the country being very mountainous.

Unlike the head before us, the one mentioned by Mr. Buckland was painted in colours. Perhaps this arises from difference in the customs of the various tribes of which the Jívaros are composed.

On Germs.

BY G. M. SMITH, M.R.C.S., L.R.C.P.

(*Abstract.*)

TO illustrate the meaning of the word "germ" two examples were given, viz.: (1) The fermentation of wort by beer-yeast, and formation by the small cellular structure, the yeast-plant, of alcohol and carbonic acid; (2) The disease known as splenic fever amongst cattle, which has been shown by Pasteur to be due to a small rod-like body, called the *Bacillus anthracis.* It is now known that many diseases, and most of the phenomena of decay, decomposition, and other important chemical changes, are initiated by these small organisms. They are almost universal, existing in air, earth, and water; they possess great powers of reproduction, and are difficult to destroy. The methods of investigation employed are: (1) powerful microscopes and appropriate staining agents; (2) cultivating them in nutrient jelly, and getting them in a pure condition (apparatus shown for this purpose, including sterilizing chambers, steaming can, plate, and test-tube arrangements, etc.); and (3) proving their effects by injecting them into animals. Under the last division we have to ascertain to what extent they are the cause, concomitant, or consequence of disease.

Specimens of growing *Anthrax, Bacillus Violaceus, Micrococcus prodigiosus*, etc., were shown; also microscopic slides of cholera bacillus, the germ of mouse *Septicæmia*, etc., etc.

The Deposition of Smoke and Dust by means of Electricity.

By WILLIAM PHILLIPS MENDHAM, ASSOCIATE OF THE SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

(*Abstract.*)

OWING to the extreme mobility of the particles of dust with which our atmosphere is laden, it is a very easy matter to displace them, as they are susceptible to very gentle currents of air, but if left to themselves will, under certain conditions, quietly settle till again disturbed; they may also be destroyed by burning.

A beam from a lantern reveals the presence of these particles, and if a hot flame or a heated body be held beneath the beam, a black space is produced across the beam, owing to their combustion by the flame and displacement by the ascending current of hot air from the heated body.

The most probable explanation of this behaviour of the dust-laden air seems to be that the air immediately above the source of heat becomes rarefied, and hence rises more quickly than that air without its influence. The dust particles are not able to rise so quickly as the rarefied air which contained them, and are consequently left behind.

As an upward stream of hot air is capable of displacing the particles, so can they be displaced by introducing into their midst a gas so prepared that in it suspended particles do not exist. Oxygen and ordinary coal gas produce the black space equally well as a stream of hot air.

It is well known that if the air in a room be left to itself for a time, and no currents or draughts of air be allowed to enter, the dust in the air of that room will quietly settle down on the articles of furniture and even upon the walls. So that could we, without disturbing in any way the confined air, shoot a beam of light across the room, instead of the track of that beam being rendered visible by the floating dust, the beam would only be seen where it fell on the opposite side.

Professor Tyndall, while engaged in his researches on Disease Germs, was by means of the luminous beam able to prove that towards the end of an expiration of air from the lungs, the air expired was entirely free from suspended matter, the lungs acting as filters holding back all particles of dust inhaled. By breathing through a tube across the track of a beam, he noticed that the beam appeared as pierced by an intensely black hole, which was formed by the deeper air from his lungs.

Absolute freedom from dust is not only obtained by means already mentioned, but it has been demonstrated experimentally that any hot body is surrounded by a film of air which is absolutely free from dust, and this hot body produces an ascending dust-free stream of air.

The displacement of the dust particles is most probably due to the same cause that produced a similar effect in the beam,—namely, the rarefaction of the air immediately surrounding the body, the dust particles preferring to lag behind and enjoy the close company of their fellows in the denser atmosphere, than to rise in the ascending current.

Very interesting is it to note that the converse of what has just been mentioned as taking place with regard to a hot body is true with regard to a cold body. To Lord Rayleigh is due the discovery that from a cold body a stream of dust-free air descends. Both these phenomena can be shown by means of the lantern in front of which a glass chamber filled with smoke has been placed. A length of platinum wire (placed on the base of the chamber) is rendered incandescent by the passage of an electric current supplying a source of heat for the first experiment; and in the second experiment iced water is passed through a metal tube against the uppermost side of the chamber already filled with heated smoke.

There is another process to which smoke and dust can be subjected with a view to their condensation or deposition, a process differing entirely from those already alluded to.

Between thirty and forty years ago an observer noted what he termed the "perfectly magical effect" produced by an electrical discharge taking place in a body of smoke confined within a bell jar. He describes his jar as resting in water in order to exclude the disturbing action of the outside air, and he allowed small quantities of smoke to enter through the top. Upon his electrical machine being started, immediate condensation of the smoke occurred.

Here the matter seemed to drop; M. Guitard does not appear to have made any further investigation as to the practical utility of his discovery for condensing objectionable and waste smoke or fume. This experiment can be performed more easily by an electrical machine such as that designed by Mr. Wimshurst, than with the appliances which were at M. Guitard's command. If a pointed wire be led from one electrode of the electrical machine (the other electrode being put to earth) to a volume of smoke as it issues from a small model furnace, the smoke is simply forced again down the flue, while the effect produced by presenting a collection of points is to condense the smoke.

If smoke be confined within a bell jar, the effect of an electrical discharge upon it can be the better observed as the smoke is in a state of perfect equilibrium, and the result of any stress set up in its midst is at once apparent.

If an electrical discharge be made to take place within the bell jar, the confined smoke becomes greatly agitated, finally disappearing, leaving a thin white film covering the whole of the interior of the jar and the electrodes. A precisely similar effect is observed if the discharge be made upon smoke in motion, for which experiment the smoke may be generated in a temporary furnace connected by means of a short flue to a glass compartment in which the electrodes are placed. The smoke may be generated by placing liquor ammoniæ in close proximity to burning sulphur or by burning zinc filings.

As to the utility of the remarkable property which an electrostatic discharge possesses over a smoke and dust-laden atmosphere, it is perhaps too early to speak; but it has been applied on a large scale at some lead works in this country, and has met with some degree of success.

It has hitherto been the custom at some works to lead the fume through miles of flue, in order to arrest and deposit the fine particles of sulphide of lead of which it is mainly composed, and it has been thought by some that could an electric discharge be brought to bear upon the fume, the deposition would be effected in a less expensive and at the same time more expeditious manner.

Various devices have been hitherto adopted to arrest the fume. In some works the fume is led through chambers specially constructed for the purpose, upon the floors of which water is allowed to stand. Other plans have been adopted, among them that of using woollen bags through which the fume is blown and by them arrested. Where the effect of the electrical discharge has been tried, the inlet of the chamber into which the fume has been led has been surrounded by points for the rapid discharge of electricity upon the smoke as it entered.

It is a question still to be solved, as to how far the phenomenon which is the subject of this paper will be applied to the requirements of every-day life.

In flour mills, where inflammability of the fine dust is a direct source of danger, the application of such an apparatus might be of considerable benefit. Again, where such dusty work as chaff-cutting is carried on close to warehouses or dwellings, it might be of advantage to use the electrical discharge for condensing or throwing down the particles of dust.

The Physical Formation of the Earth.

By S. W. TYACK.

FROM times of remote antiquity wild speculations concerning the origin and physical form of the earth have been handed down to us, through Hindoo poetry, Egyptian hieroglyphic, and a hundred other channels; these all showing that the ancient idea was that the earth was flat, or almost so. Careful investigation proved, however, to unbiassed minds the fact, now never called in question, that our world is spherical.

The nebular hypothesis of Laplace (*Mécanique Céleste*, 1799) was the first and most successful attempt to explain the actions which called the earth into being. According to his theory, our planet, together with the other members of the solar system, was originally part of the sun, then existing as a huge mass of highly heated vapour revolving with enormous rapidity about its axis. Portions of its gaseous substance it flung off from itself by centrifugal force, and these, under the attractive influence of their parent mass, commenced to circle round it as satellites. This theory is now in the main accepted, though it is considered probable that the earth was not in a gaseous, but liquid condition when it began its separate existence.

There are many indications of the probable fluidity of the globe at one time—the appearance of some geological strata, natural crystalline formations, the estimated present temperature of the earth's interior,—these may be taken as examples. And accepting this, it may be experimentally demonstrated that a liquid mass would assume the oblate spheroidal form taken by the earth when subjected to the forces which acted on the latter. Liquid masses when freed from the action of the earth's force, form themselves into spheres on account of the mutual attraction existing between the particles of which they consist, as the sphere is the figure which will give the least possible average intermolecular distance. Oil suspended in a mixture of alcohol and water of its own specific gravity can be used to show this. To complete the imitation of the formation of such a spheroid as the earth, the spherical oil-globule should be rotated, when it will flatten in a direction perpendicular to the axis of rotation, in obedience to the first law of motion as enunciated by Newton. If a very rapid rotatory motion be communicated to the globule, portions will be flung off (as Laplace supposed did happen in the case of the sun) and will form smaller globules.

Various stages of formation by a process similar to that which produced our earth are exhibited in the other planets of our system, and from these it is apparent that the polar flattening varies with the rate of rotation and the greater or less plasticity of the masses (as can be further shown by experiment).

From the amount of the earth's compression ($\frac{1}{299}$ of its equatorial diameter), it has been attempted to estimate its age. Its rate of revolution is decreasing, probably owing to tidal friction. Supposing that this has remained constant, then 1,000,000,000 years ago it was revolving on its axis at double its present speed (Adams). If the earth consolidated at or before this time, its ellipticity must have been at least $\frac{1}{280}$, and therefore its solidification took place at a considerably later date (Thomson). Darwin, however, states that sub-aërial denudation renders this method of estimation unreliable. From the law of cooling, 10,000,000 years has been calculated to be the time elapsed since the consolidation of the earth's surface (Thomson). Darwin objects to this also, inasmuch as, according to him, tidal friction will have generated sufficient heat to materially augment the length of time taken by the earth to reach its present temperature.

On Colour-Blindness.

By Prof. W. RAMSAY, Ph.D.

(Abstract.)

IT has been suggested as a possible explanation of the inability of certain persons correctly to distinguish certain colours, that it is only recently that the human race has acquired an appreciation of colour; and Mr. Gladstone, to whom I believe this remarkable theory is due, quotes the ancient Greeks as an instance of this curious lack of perception, stating in support of his view that their nomenclature for colour is defective; that one and the same word was used to express what now-a-days people regard as totally distinct colour-perceptions. But Mr. Gladstone confuses two wholly different things: the ability to perceive and the power of naming. The Celtic languages have even now no distinguishing names for blüe and grey, the same word serves for both; but yet it is absolutely certain that Celts know these colours, as is proved by their capacity of distinguishing between them, and of naming them as soon as they learn a language in which each has a separate name. It might as well be doubted that the sense of smell of the whole human race is imperfect because it is all but impossible to describe smells, and that purely for lack of nomenclature.

The usual explanation of the inability of certain persons to distinguish certain colours is based on the assumption that the human eye is sensitive to three and only three colours, which are therefore named "primary." Regarding the actual colours to be considered as primary there has been some dispute. But if this theory is accepted, its expounders explain colour-blindness by the theory that colour-blind persons are wanting in the ability to perceive some one of these primary colours.

Now there is absolutely no physiological basis for the assertion that the eye is sensitive to three colours, and that all others are compounded of these three. If it be true, then perception by the eye is an exception to all other methods of receiving external impressions. The ear is sensitive to vibrations of the auditory nerve produced by the impact of air-waves. The skin is sensitive to heat vibrations. It has been pointed out by the author in a paper published in the *Proceedings* of this Society some years ago, that in all probability the sense of smell is due to vibrations of the olfactory nerves; and Professor Haycraft has recently speculated on a similar cause for taste impressions. Now we know that as to the sense of hearing there is a limit to the number of vibrations per second detectable by the ear; also that some ears are more sensitive to high notes than others, so that a sound which impresses one person as a musical note is inaudible to another, whose ears are incapable of perceiving the sound of such rapid vibrations. The skin, in its ability to perceive heat or cold, is also insensitive to extremes of temperature; so that a scar from a bright-red-hot piece of iron is almost painless, and is not perceived as heat; nor does an extremely cold object produce a cold sensation. In all probability, too, the inability to smell gaseous substances of low molecular weight is caused by the organs of smell being insensible to the too rapid vibrations of the simpler molecules.

Now every possible tone between the extreme limits of audition can be heard by the ear. It cannot be said that all tones are derivable from a few fundamentals; for although fundamentals may yield overtones, yet these overtones are perceived by us as modifying the quality of sound (Helmholz) much more frequently than as the actual tone heard as primary.

So too every possible temperature between the limits of our heat-perceptions is perceived by the skin; and it is also probable that every possible variety of smell is perceivable as such by the olfactory nerves; and that the smells which we perceive are not compounded from a few elementary perceptions. We are acquainted with homologous series of chemical compounds, in which the smells progressively change with progressive alteration in the molecular weights, and yet no one smell can be declared a mixture.

Reasoning from the analogy of the other senses, therefore, it may be concluded that the optic nerve is sensitive to all possible vibration between certain limits, and not merely to three or four kinds of vibrations, each kind having its own special period.

Another argument for this view may be drawn from probability. Suppose a young child to be sensitive as regards red to some absolutely definite number of vibrations, and to none other so far as the colour red is concerned. Now, it is unlikely that such a child's perception will remain stationary, for the nerve fibres will enlarge with the growth of the body. It would therefore appear that adults should have totally different colour-perceptions from children, and I am not aware that this is the case; I should certainly deny it as regards myself. It might be well, however, to sets the limits of vision of children more completely than has yet been attempted.

Two cases of colour-blindness have fallen under my notice, and although it would be improper to rest a theory on such a very limited basis, yet observations, so far as they go, support the explanation stated.

The first case is that of a gentleman who could not name any colour but blue. On being given an assortment of coloured wafers to arrange, he first singled out all blue ones and also a number of pink wafers, saying that these were all blue. The others he arranged, without any reference to colour, in an order which appeared to him to represent difference of shade; dark chocolate, red, and green being placed indiscriminately in one class, orange and light green wafers forming another, yellow and light grey a third, and white the last. To all appearance these colours struck his eye as more or less dark shades of grey. In monochromatic sodium light he arranged the colours in the same way, and said that to him they presented no change of appearance. He was incapable of distinguishing any colour in polarised light, and indeed denied that there was any difference in intensity of shade. Now to a normal eye, blue appears darker, *i.e.*, less illuminated than yellow; but of course it is difficult to compare the relative shades of two wholly different colours. This gentleman was able to perceive light at the violet end of the spectrum where a normal eye could see nothing; and his vision was much restricted at the red end. He could see a solution of sulphate of quinine in gaslight as coloured, and named it blue; by an ordinary eye the solution cannot be distinguished from water in artificial light.

The other case which I examined was a more usual one. The subject was unable to distinguish green from red. It appeared, however, that a red object held close to the eye was recognised as coloured, but at a distance the shade was simply dark. It matched with a deep grey. I think that this eye saw red at a distance merely as non-illuminated, and that a red object among green was invisible, simply owing to its appearing dark and like the deep shadows of the green. I had no opportunity of testing this case with a spectroscope.

The most striking confutation, it appears to me, of the theory that colour-blindness is owing to the absence of perception of one or more of the so-called primary colours, is furnished by the case of a celebrated engraver, who was absolutely incapable of colour-perception of any kind. Now if the perception of colour depends on the existence of three sets of colour nerves, and if all three are inactive, it surely follows that light itself will not be perceived. But if in such an abnormal case the eye is sensitive to light of one period of vibration only, the effect will be that monochromatic light and total colour-blindness will result.

It is, of course, always conceivable that the particular defect which causes colour-blindness lies in the brain, and not in the eye. Certain persons are incapable of judging which of two musical tones is the higher, even when they are more than an octave apart. Yet such persons hear either tone perfectly; the defect is not one of deafness. It must be concluded that in such a case the brain is the defaulter. And it may equally well be the case that the inability to perceive certain colours is not due to a defect in the instrument of sight—the eye, but to the power of interpreting the impressions conveyed to the brain by the optic nerve. If this is the case, the problem is no longer a physical one, it falls among those with which the mental physiologist has to deal.

Five Weeks at the Zoological Laboratory at Roscoff.

By J. G. GRENFELL, M.A., F.G.S.

(*Abstract.*)

THE French Government maintains several stations on its coasts for zoological research. Roscoff, in Brittany, is one of these; others are at Villefranche, near Nice, and Banyul, at the foot of the Pyrenees. These are all under the direction of M. Lacaze Duthiers, and are most liberally opened to foreigners. At Roscoff the sea retreats a great distance at low tide, leaving exposed rocks, loose stones, sand, and mud. The Ile de Batz affords protection from the worst gales. Consequently the marine fauna there is extraordinarily varied. The lecturer described some of the more interesting forms met with. In mud and sand many fine Annelids were found, such as *Phascolosoma*, *Siphunculus*, and *Sabella*. *Synapta*, one of the *Holothurians*, was abundant in sand. In rock pools many small crabs had the strangely metamorphosed crustacean parasite, *Sacculina*, under their tails. The pretty crinoid, *Comatula*, was common on a particular kind of sea-weed, which it closely resembled. A very small worm, *Convoluta*, was interesting because its bright green colour was due to chlorophyll, the

green colouring matter of plants. Ascidians of many kind abounded; amongst these *Molgula* and *Phallusia* were especially fine, and *Clavellinas* showed the reversing action of the heart. Amongst the Mollusca fine *Myas*, *Clams*, *Pectens*, and *Cypræas* were found. *Octopus* and *Sepia* were not uncommon. Expeditions were made by boat to isolated rock stacks at very low tides. The fauna here was wonderfully rich. Sea anemones, sponges of many kinds, ascidians, and the hydroid polyp *Lucernaria*, were found in extraordinary numbers; on the sea-weeds were many diatoms and live foraminifera. From these rocks came also the splendid barnacle *Pollicipes*, which has thick, strong valves, and lives exposed to the wildest dash of the waves. Among the sponges was one specially fine one, *Pachymatisma*, which has a sphincter muscle round each oscule.

Many interesting forms of *Polyzoa* were found; such as *Loxosoma* and *Pedicellina*, the former parasitic on the tails of the gephyrean worm *Phascolosoma*. Internal parasites were very numerous; many *Gregarines* were found, one possessed of much more active powers of locomotion than has hitherto been ascribed to these little creatures. A new infusorian, a marine *Urceolaria*, was found. The *Dicyemida* were abundant in the *Octopus* and *Sepia*. The lecturer had succeeded in killing these parasites beautifully, by suspending a bit of the kidney of the molluscs in weak chromic acid. Thus prepared they were almost entirely free from mess of all kinds, and could be kept for years in liquid, and mounted in balsam at any time.

Reports of Meetings.

GENERAL.

DURING the past Session there have been eight General Meetings of the Society, all held in University College, at 8 p.m.

On Thursday, October 7th, 1886, Dr. Burder exhibited an apetalous variety of the wood-sorrel (*Oxalis Acetosella*). Prof. Leipner showed (for Mr. F. F. Tuckett, of Frenchay) a *white* variety of the common blackberry, a single plant of which had been found growing in an old quarry in the neighbourhood. He reminded the Society that a similar plant had been found some years ago in a field near Winscombe, and that during one of the botanical excursions in 1885 this plant had been seen by several members of the Society. Prof. Sydney Young, D.Sc., read a paper on "The Distillation of Coal, Coal Tar, the Colouring Matters, and other products obtained from it." One of the most recently discovered and interesting products is a substance named *Saccharin*, which is about two hundred times as sweet as sugar, has antiseptic properties, and is not poisonous.

On November 4th, Mr. Phibbs exhibited a "sacred war-trophy" from Ecuador. The account of it will be found at page 183. Prof. Leipner gave an account of the germination of a *cactus* within the fruit, the young plant being green, though not exposed to light. Mr. G. Munro Smith, L.R.C.P., M.R.C.S., read a paper on "Germs," an abstract of which will be found at page 186.

On December 2nd, Prof. Ramsay, Ph.D., read a letter from Prof. Raphael Meldola, secretary of an association for promoting the investigation of various phenomena on which widespread observations are much needed. It was suggested that members of the Society should assist in work on the following subjects, selected from a long list:—(1) Electrolysis; (2) Tidal Phenomena; (3) Meteorology; (4) The Erratic Boulders of Great Britain and Ireland; (5) The Circulation of Underground Water; (6) The Migrations of Birds; and (7) The Disappearance of Native Plants from their Original Habitats. Mr. H. Charbonnier exhibited preserved specimens of the "Grey Phalarope" and the "Little Gull." Mr. Edward Wilson, F.G.S., showed living specimens of the large South American land snail, *Bulimus Oblongus*. The diameter of the shell is about four inches. Empty shells of some allied species were shown for comparison; and also some of the oval, white, calcareous shells of their eggs, which are about one inch long. Mr. W. P. Mendham read a paper on "The Deposition of Smoke and Dust by means of Electricity." An abstract of this will be found on page 187.

On January 6th, 1887, Mr. H. Charbonnier read "Notes on the Reptiles, Amphibia, and Fish of the District." This is printed at page 133. Mr. Ll. Tyack read a paper on "The Physical Formation of the Earth." A short account of this is printed at page 192.

On February 3rd, Prof. Ramsay, Ph.D., read a paper on "Colour Blindness," which will be found at page 195.

At the meeting on March 3rd, Mr. Thomas W. Jacques read some "Notes and Observations," in which he drew attention to several points in natural history which need elucidation, and urged all the members to record their observations thereon, and to bring them before the Society from time to time, undeterred by the common idea that short and, perhaps, disjointed records were out of place and not acceptable. Dr. Burder then read a most interesting communication on the "History of the Potato." This is printed at page 165.

On April 2nd, Prof. Lloyd Morgan, F.G.S., read a paper on the "Senses and Sense Organs of Insects," which will be found at page 178.

At the last general, which was also the 25th annual, meeting, the Report of the Council was read by Prof. Ramsay, the balance-sheet was presented, and officers for the ensuing season were appointed. Prof. Leipner then read some notes by Prof. Lloyd Morgan, F.G.S., on the following specimens from West Africa: (1) *Calamoichthys Calabaricus*, a Ganoid fish, one of the Polypteridæ, resembling many of the fossil fish of the Old Red Sandstone and carboniferous rocks. (2) *Malapterurus Electricus*, the electric cat-fish, one of the Siluridæ. In it the electric organ extends over the whole body, but is thickest on the abdomen. It consists of rhomboidal cells which contain a rather firm gelatinous substance. The nerve-supply seems to be peculiar; and it is of interest to note that in the three electric forms—the Torpedo, the Electric Eel (*Gymnotus*), and this Electric Cat-fish—each has a different mode of innervation for the "battery." In the first the supply is cranial in origin, each organ receiving one branch from the "trigeminal" nerve, and four branches from the "vagus." In *Gymnotus* the apparatus is supplied with more than two hundred nerves, which are spinal in origin. In *Malapterurus* the nerve is spinal, and consists of a single enormously strong primitive fibre, which distributes its branches in the electric organ. This difference of nerve-supply seems to show a different origin for the electric organs, similar as they are in ultimate structure in these different forms. (3)

A large scorpion (*S. Africana*). (4) A chameleon (*C. vulgaris*). (5) Some large centipedes (*Scolopendra*).

Mr. J. G. Grenfell, M.A., F.G.S., then read a paper, entitled "Five Weeks at the Zoological Laboratory at Roscoff," an abstract of which will be found on page 200.

ARTHUR B. PROWSE,
Honorary Reporting Secretary.

CHEMICAL AND PHYSICAL SECTION.

DURING the past Session only four meetings of the Section have been held.

Papers have been read by the following gentlemen:—Dr. Richardson, Messrs. W. A. Shenstone and J. T. Cundall, Prof. W. Ramsay, Dr. S. Young, and Mr. A. P. Chattock.

SYDNEY YOUNG, *Hon. Sec.*

ENGINEERING SECTION.

THE inaugural meeting of this Section was held on the 21st December, 1886. Seven meetings have been held, and papers have been read by the following gentlemen:—The President (Mr. Charles Richardson), Mr. Thomas Morgans, Mr. J. W. I. Harvey (Vice-President), and Mr. G. W. Sutcliffe. At the 7th meeting, held on June 28th, a most interesting paper was read by Mr. Richardson, on "The Severn Tunnel," a work which he originated and assisted, as joint engineer with Sir John Hawkshaw, to carry out. To this meeting members of the Naturalists' Society generally were invited.

NICHOLAS WATTS, *Hon. Sec.*

ENTOMOLOGICAL SECTION.

THE usual meetings of the Section were held during the winter, with the exception of one arranged for March 15th, but which was prevented by the heavy snowfall.

At the December meetings, Mr. G. C. Griffiths exhibited a singular little species of wasps from Florida, with a group of pitcher-shaped cells.

The Secretary exhibited a specimen of *Clytus annularis*, an Indian species, which was taken at Margate.

A very large number of species of different orders have been exhibited at the different meetings of the Section by a number of members, including many fine and rare species, both British and exotic.

GEO. HARDING, *Hon. Sec.*

GEOLOGICAL SECTION.

THREE evening meetings of this Section have been held during the past Session, when Professor C. Lloyd Morgan read papers: (1) *On Upheaval, its Causes and Effects;* (2) *On the Origin of Mountain Ranges;* (3) On his recent experiments upon *The Crushing Stress and Weather Resistance of Local Building Stones.*

ALFRED C. PASS, *Hon. Sec.*

MICROSCOPICAL SECTION.

THE Section has met regularly during the past winter and spring. No papers have been read, but various methods of microscopical mounting have been demonstrated.

C. K. RUDGE, *Hon. Sec.*

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Dolomitic Conglomerate of Bristol.

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IN the neighbourhood of Bristol the Dolomitic Conglomerate will be found lying against the upturned edges of the Carboniferous Limestone in nearly every case, the chief exceptions being from Sneyd Park towards Portishead, and along the coast from Portishead to Clevedon, where it lies against the Old Red Sandstone.

The Avon, in its course from Bristol to the Severn, runs successively through the Carboniferous Limestone, the Old Red Sandstone, the Dolomitic Conglomerate and the Keuper, or Red Marl, from which it emerges near Shirehampton into the alluvium bordering the Severn. A peculiar development of the Dolomitic Conglomerate is found at the village of Almondsbury, which is situated almost entirely upon it, where it swells out from a narrow strip to quite a large area. At this place there used to be worked a rather fine quarry of the Conglomerate; and some fine blocks from thence were used in the building of the bridge under the South Wales Union Railway near Cattybrook Farm. Its extremely partial and local development was peculiarly shown in the construction of the new tunnel on the before-

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mentioned railway through Almondsbury Ridge. The distance apart of the old and new tunnels, from centre to centre, is not much over forty feet, and between their sides not more than twenty feet; and yet during the construction of the first tunnel, the Conglomerate was not found, or at any rate noticed to be there, nor is it shown on the geological section taken during its construction. It seems hardly likely that it would thin out or suddenly disappear in such a short distance; and yet such has been the case, or, at any rate, to a very great extent. It is quite possible that a small portion may have been passed through unnoticed in constructing the tunnel, and may have been omitted from the section. The Conglomerate found, however, in the new tunnel is sixteen feet in thickness, and was amongst the hardest of the rocks found in the tunnel, and gave the most trouble. It is certainly most strange, therefore, that it should have been passed through unnoticed. The top of the Conglomerate found in the new tunnel runs nearly level for about two hundred yards, from whence it suddenly rises at nearly as great a dip as the Carboniferous rocks, over the edges of which it passes. The only explanation that appears probable of the absence of this Conglomerate in the old tunnel, the bottom of which is about on a level with the crown of the new tunnel, is, that when it makes its sudden rise over the edges of the older rocks it comes to a sudden end, and therefore did not rise up to the level of the old tunnel, or at any rate was only met with slightly in its bottom, and therefore remained undistinguished from the following beds of hard grit. It consisted chiefly of particles of Millstone Grit, some of them of considerable size and exceeding that of a man's head, together with pieces of Carboniferous Limestone, but chiefly of a much smaller size. Towards its junction with the Carboniferous rocks it was composed chiefly of fine crystals, very yellow in colour, and which crumbled into a fine crystallized sand on being cast out on the tip. North of Bristol, the Dolomitic Conglomerate is not so largely developed, even against the Limestone ridge running from Olveston, *viâ* Wickwar, to Chipping Sodbury; and is only found in small detached portions, excepting near Thornbury, where there is a considerable area lying partly against the Old Red Sandstone, and partly against the Carboniferous Limestone. A peculiarity here is, that you have a clear way from the Severn Alluvium to the Dolomitic Conglomerate without passing over the Keuper Marl, and that, passing in a south-easterly direction, the strata alternate from Dolomitic Conglomerate to Old Red Sandstone, Dolomitic Conglomerate again, then Old Red again, from that passing on to the Carboniferous Limestone, after which the Dolomitic Conglomerate again appears, succeeded by the Keuper Marl.

The Dolomitic Conglomerate of our district is an eminently local deposit. This is well seen in the road cut from the Port and Pier Railway Station, under the Suspension Bridge, to the Downs near Proctor's fountain, where blocks are found of very large size, and weighing several hundredweight at least, showing that the source of supply was close at hand. These blocks chiefly consist of Carboniferous Limestone and Millstone Grit, and are embedded along with small pebbles.

A similar instance to that of Patchway Tunnel, before referred to, of the sudden rise of the Dolomitic Conglomerate from a comparatively horizontal position to a steep rise, is shown in the section of the Severn Tunnel, published in the last volume of the Proceedings of the Bristol Naturalists' Society. There it is first met with at the bottom of No. 2 shaft, whence it passes, with a slight undulation about half the distance towards No. 3 shaft, forming nearly the bottom of the tunnel for about that distance; it then takes a quick rise over the Carboniferous rocks to No. 3 shaft, where it was found about twenty-six feet in thickness, afterwards thinning out to four feet at No. 4 shaft, on the Monmouthshire shore of the Severn. This is very similar to Patchway Tunnel, although it was not found to disappear in so short a distance, but shows the erratic manner in which the deposit was formed. It again appears above low-water mark on the edge of the shoots, forming what is called the "Lady Bench." It would be interesting to know to what extent it is developed between that point and Almondsbury Ridge.

South of Bristol, the Dolomitic Conglomerate is found much more general than to the north, forming an almost complete fringe to the Limestone rocks of the Mendip Hills. The road from Bristol to Congresbury, between Flax-Bourton and the latter place, runs for the greater part of its length through the Conglomerate, which fringes the border of Backwell Hill and Wrington Warren. From near Congresbury it takes an almost easterly direction, through Wrington to Red Hill, whence it takes a north-easterly direction to Winford, where it ends, forming nearly a complete circuit round the spur, the only exception being between Winford and Barrow Gurney, where the lower lias clay runs directly upon the Mountain Limestone, forming a kind of bay.

The most extensive development of the Conglomerate in the neighbourhood of Bristol is, however, round the main Limestone range of the Mendips, where, starting from East Harptree to the north of the range, it is possible to follow, almost without losing sight of it, in a north-westerly direction to Sandford near Banwell; and afterwards in a south-easterly direction from thence, *viâ* Axbridge and Cheddar, to

Wells and Shepton Mallet, a distance of about twenty-five miles, keeping on the Conglomerate nearly the whole of the distance, without once leaving it. A curious instance of the way in which the Dolomitic Conglomerate follows the range of the Mountain Limestone, is shown at what is known as the Shut-shelve, between Winscombe and Axbridge, where the spur, running out from Cheddar to Compton Bishop, which has an average width of about a mile, narrows suddenly on the south side to a width of scarcely a tenth of that, and is followed almost completely round by the Conglomerate on both sides of the ridge. Between Sandford (up to which point the Limestone ridge from the direction of West Harptree follows a north-westerly direction) and Winscombe there is a gap of about a mile and a half to the south, during which the Sandford spur returns again in the direction of West Harptree, with an average width of little more than half a mile for a distance of about six miles, returning again to the Shut-shelve, having performed a circuit of about twelve miles, although the extreme distance between the points is only the mile and a half before stated.

The Limestone in this inlet is protected from the following up of the Dolomitic Conglomerate, which bridges over the gap of one and a half miles between Sandford Hill and the Shut-shelve, spreading out in the direction of the inlet for about half the distance, where it lies against the Old Red Sandstone, which completely fills up the remainder of the inlet in the Limestone, and forming what is known as Black Down. One of the small gaps left in the circuit of the main Limestone ridge of the Mendips referred to, is at the Cheddar Cliffs gorge, where a gap is left, by the thinning out of the deposit, of about a quarter of a mile, through which the Mountain Limestone runs directly into the Red Marl, on which the town of Cheddar stands.

The next considerable area of the Conglomerate in a south-easterly direction, is between Draycott and Wookey. It lies between the Cheddar Valley Railway and the Mendips, the line in no case touching it, but running through the Red Marl until near Lodge Hill station, where it runs through a sudden spur, again leaving it for the Red Marl towards Wells. At Draycott there is rather a fine quarry of the Dolomitic Conglomerate, from which some capital building stone is obtainable, and of which the front of the Bristol Joint Station and other works in connection therewith were built, and which is said to withstand the Bristol acids better than most of the stone used in the buildings of Bristol. Many of the buildings on the Cheddar Valley Railway are also built of the same material. Near Draycott are three or four peculiar isolated pieces of Mountain Limestone, standing together in a group, more than a mile away from the main body, in the midst of the river Axe Alluvium, and which are also nearly, if not quite, surrounded by a belt of Dolomitic Conglomerate. The railway runs through the mid-distance between them and the Conglomerate fringing the main body of the Limestone range. There are also peculiar small isolated portions of Limestone at Lodge Hill and near Wookey, although they stand in the midst of Red Marl, and not, as in the case at Draycott, in the midst of Alluvium. There is a narrow strip of Dolomitic Conglomerate near the eastern edge of that at Lodge Hill, but none near Wookey, although in the latter case it stands very near the edge of the Conglomerate running out from the main body near Lodge Hill Station.

There is a large extent of Dolomitic Conglomerate near Chilcompton and Stratton-on-the-Fosse, on the northern side of the Mendip range, measuring about three miles in length from east to west, and one and a half mile in breadth from north to south. At the south-western end of this track, near Binegar, the Mountain Limestone and the Coal-measures are separated by a narrow neck of Conglomerate, less than a quarter of a mile across, which is also divided again from a spur by a narrow strip of Millstone Grit, which also helps to bridge over, at this point, the space between the Coal-measures and the Limestone.

The Bath and Evercreech Railway, between Chilcompton and Binegar, runs through the Conglomerate for about two miles; and some very good sections are laid bare in some of the cuttings, particularly near the bridge on the road from Wells to Chilcompton. As far as the author remembers, there were no boulders found in these cuttings; but the Conglomerate was composed of pebbles up to about two inches in length, cemented together very closely, and very dark in colour. Although the before-named railway crosses the Mendips very near their highest point, this was the only Conglomerate passed through in its entire length of twenty-six miles, and it almost escaped altogether, only passing along the north-west edge of the strata. It is worthy of note that this railway throughout, although rising upwards of 700 feet over the Mendips, passed for twenty-one miles through Oolitic and Triassic strata.

A friend of the author's recently, in building a house near the foot of Worleberry Hill, on the south side of it, at Weston-super-Mare, in excavating a site for a stable through what was expected to have been Limestone, came upon a capital bed of Dolomitic Conglomerate of the Clevedon type, from which he had the house built, instead of from the Mountain Limestone, as had been intended. The Conglomerate is very light in colour, and, at a distance, the house has the appearance of being built of the Inferior Oolite.

At Mells, which is now a station on the Bristol and Radstock and Frome Branch, we find rather a curious development. The Dolomitic Conglomerate lies partly against the Millstone Grit and partly against the Mountain Limestone, where the Millstone Grit thins out and disappears, but the Conglomerate is succeeded to the north by the Inferior Oolite, which lies directly against it. This Inferior Oolite is an extension of the same formation which caps the hills on both sides of the valley through which the beforementioned Bath and Evercreech Railway runs, and which it follows all the way from Bath to Radstock, the Oolite from thence taking the direction of Mells, where it abuts against the Dolomitic Conglomerate referred to.

The salinity of the waters of the old Triassic lake in which the Keuper Marls succeeding the Dolomitic Conglomerate were deposited, is shown by the occasional pseudo-morphs of salt (as at Aust Cliff), and by the interstratified bands of Gypsum. By referring to the section of the new tunnel through Almondsbury ridge, it will be seen that beds of Gypsum were rather extensively found throughout the Red Marl, up to four inches in thickness, but disappearing on approaching the Dolomitic Conglomerate. The Gypsum beds were considered so extensive that overtures were made, by a local mining prospector, to the railway company for permission to work them, and also to sort that already brought to the surface; but, after the terms were agreed upon, the matter fell through, owing probably to the adventurer (as they are called in Cornwall) not finding the market he expected for the proceeds, or that, after maturer consideration, the prospects of finding a remunerative percentage of the mineral did not seem so good as at first sight.

A strongly marked and violent unconformability presents itself everywhere between the Triassic deposits and the older formations on which they rest. The unconformability of the Permian upon the Carboniferous, although well marked, cannot be said to be violent; for, as a rule, the Permian strata have the same, or approximately the same, dip as the Carboniferous when the two formations occur together. But the Triassic strata bear no such relation to the formation immediately preceding them. All the palæozoic strata had been upheaved, dislocated, metamorphosed, and extensively denuded before the Triassic deposits began to be laid down. These latter circle round the outskirts of the present high grounds of Wales and the North and South-west of England, and overlap the Carboniferous strata in such a way as to lead to the inference that the configuration of Wales, and Northern, Central, and South-west England in Triassic times had even then assumed much of its present appearance.

Varieties of Ferns in the Bristol District.

By Col. ARTHUR M. JONES.

I HAVE been asked to furnish a few notes on the Varieties of Ferns known to have been found in "the Bristol District," and to add some remarks on varieties of ferns generally, and more especially on their classification, it being now generally accepted that the varieties of ferns range themselves naturally in certain groups.

The list produced does not profess to be in any way exhaustive, it comprises simply the varieties that have come under my own notice, and it is supplied in the hope that it may be the means of bringing to light many unrecorded discoveries, so that eventually a complete list of the ferns of the district may be compiled. In the subjoined list the species are taken merely in the order in which they would appear to have been most productive of varieties:

SCOLOPENDRIUM VULGARE.

Var. Crispum. Four plants of this variety were found many years since by Dr. F. Brittan near Hanham, on the opposite side of the river Avon, and Mr. Sherring has recently reported that a young plant was found in a wood near Hallatrow.

Var. Cornutum (Leipner), an unique fern, found some years since on a wall at Long Ashton, by Mr. A. Leipner, and described by him as being undulate with crenate margins, and with the last half-inch of the midrib leaving the plane of the frond and projecting horn-like from its upper surface.

Var. Peraferens. This remarkable form, in which the frond is truncate, terminating in a distinct pouch at the back of the frond, while the midrib is prolonged for an inch or so, like a horn, was found by myself in considerable quantities in a wood at Portishead, and is no doubt still to be found there; four plants of the same variety were also found by me at Westbury-on-Trym.

Var. Undulatum. A good form of this variety was found by Mr. G. B. Wollaston, near Pill, about ten years ago.

Var. Fissum. An unusually good form was found by me in Leigh Woods about twelve years ago; also a very good form, about the same time, at Westbury-on-Trym.

Var. Variabile. Unusually good forms of this very peculiar variety have been found at Westbury-on-Trym.

Var. Laciniatum, more than once in the same neighbourhood.

Var. Multifidum. Many fine forms of this have been found at Westbury-on-Trym, Portishead, and other places.

Var. Marginatum. A good form of this was found by me in company with Mr. Wollaston near Pensford.

Var. Submarginatum. This has been found in some quantity in a wood near Kingsweston and in other places.

Var. Supralineatum is recorded to have been found three times, at Pensford, Wraxall, and Cheddar.

Var. Ramosum was found near Olveston.

Var. Marginato-cornutum. A very fine form was found near Wells, and is now in the possession of Canon Ellacombe.

POLYPODIUM VULGARE.

Var. Subcristatum was found about fifteen years since in Leigh Woods.

Var. Semilacerum. Many good forms of this have been found there also.

Var. Semilacerum deltoideum. A very marked form of this also.

Var. Variabile. This also.

Var. bifidum. At Portishead.

Var. bifido-furcans. At Portishead.

LASTREA DILATATA.

Var. Crispato-cristatum. One of the finest crested forms of Dilatata was found about twelve years since beyond Failand by Oscroft, a travelling fern-hunter.

POLYSTICHUM ANGULARE.

Var. Decompositum. Many good forms of this have been found in different parts of this district.

Var. Tripinnatum. A well-developed form of this I found near Congresbury.

Var. Interruptum. Do. do.

ASPLENIUM RUTA-MURARIA.

Var. Laciniatum, found by Mr. G. B. Wollaston on St. Vincent's Rocks.

Var. Cristatum. Do., on the Leigh Road.

ASPLENIUM ADIANTUM-NIGRUM.

Acute and obtuse forms have both been found.

CETERACH OFFICINARUM.

Var. Crenatum was found many years since at Kingsweston.

Thus it will be seen that though the Bristol district cannot be regarded as exceptionally productive in varieties, it has contributed in an unassuming way its quota of varieties, among which are more than one that will hold a permanent place in all good collections of British ferns. But if the area of research were extended beyond the somewhat arbitrary bounds which in this case geology has assigned to it, and it were allowable to include the remainder of the counties of Somerset and Gloucester and the county of Monmouth, which may fairly be considered as naturally belonging to the district of which Bristol is the centre, some of the most productive districts in England would come under our notice.

The Forest of Dean, and still more the county of Monmouth, are rich in varieties of Scolopendrium, and particularly in the finest form of all, crispum; nor do I believe that I go too far in saying that Monmouthshire has produced nearly as many specimens of this beautiful variety as the rest of England altogether; and as this variety is barren, except in very rare instances (none of which are applicable in this case), this productiveness is clearly due less to accident than to some local peculiarity. Some of the Monmouthshire forms of crispum ("robustum," "serratum," "fimbriatum," "latissimum," etc.) are among the finest of their class. This variety, owing to its barrenness, is generally found in single plants, or two or three together at the outside, and the discovery of half a dozen plants by the same hunter is generally considered the work of a life; but by singular fortune I once found twelve plants in one lane in Monmouthshire, and on another occasion seventeen plants on a bank in a different part of the same county, all barren. Several districts in Somersetshire (Silverton, Wiveliscombe, Chard, Frome, etc.) have been more than ordinarily productive of this variety, and that very distinct form, laceratum (or endivifolium), was found at Taunton; and it was in the lower parts of the Quantock district that the late Mr. Elworthy made, many years ago, those remarkable discoveries in Polystichum angulare which helped to give a greatly increased interest to the study of British ferns, and subsequently Mr. G. B. Wollaston, Mr. Padley, Dr. Wills, and myself found many fine and distinct varieties in the same district. Nor may it perhaps be without interest to bear in mind that it was in this same district, which formed Mr. Elworthy's happy hunting ground, that Mr. George Perceval made his remarkable discovery of Devonian corals, not less beautiful than geologically interesting. It would thus seem to be proved that the affinity between this part of Somerset with South Devon, where so many of the finer forms of P. angulare have been found, is not superficial. Nor may it be unworthy of notice also that that energetic discoverer Mr. W. H. Phillips, of Belfast, has proved by his researches that a certain marked botanical affinity exists between the south-west of England and Ireland, the north more especially. There are certain marked forms of P. angulare, of which single plants had been found in the west of England, and which, having, after very exhaustive researches, never been found in any other part of England, had long been classed among the forms peculiar to the south-west; yet after all this had long been comfortably settled, Mr. Phillips turns up with his inconvenient discoveries and unsettles everything; if there were two ferns which had earned the character of having been entirely unique, those ferns were

P. angulare rotundatum of Elworthy, a Somerset form, and P. ang. acrocladon, found by Mr. Mapplebeck in South Devon; Mr. Phillips produces unmistakable counterparts of both in Ireland; another most rare and marked form, P. ang. brachiato-cristatum, which long experience had seemed to have proved conclusively to be peculiar to the south of England, Mr. Phillips also finds in the north of Ireland. It was exactly the same with another rare and beautiful form, "cuneato-setosum," of which only two plants had ever before been found, one by Mr. Moly, the other by Mr. G. B. Wollaston, and both in the south-west of England. Also the very extreme form of P. ang. grandiceps (of which two plants, apparently identical, had been found by different people in different parts of Ireland), was found more than once by Mr. J. Moly in Devon. The grand form of P. ang. polydactylum, found by the Rev. C. Padley in the Vale of Avoca, was long considered an unique plant, until I found, in the south of England, one so closely resembling it, that the seedlings of the two plants are often scarcely, if at all, distinguishable. As other instances of the rediscovery in Ireland of plants previously considered in England unique, that of Lastrea f.-mas. Barnesii deserves to be recorded; it is well known as one of the most distinct forms; yet the late Dr. Moore, of Glasneven, found in Co. Kerry a plant so closely resembling it in every way that Mr. Barnes himself, than whom a keener observer never lived, could see no difference between the two; and Mr. Tyerman found in Ireland a plant of Athyrium f.-f. plumosum (that most ladylike of all the lady ferns), so very like the original Lancashire plumosum, that only a *very* keen judge could detect a difference. Thus Nature herself having in this, as in so many other ways, linked Ireland to us so closely in every branch of natural history, shall we not all breathe a prayer—more than that, resolve—that nothing shall ever sever the connexion?

As regards the classification of varieties, the following table will show (1) the generally accepted subdivision and (2) the extent to which discoveries have been made up to the present time in the more important species; and though, in some of the species the chain of evidence is not so complete, enough is already known to show that the same principle applies, and that it is only a question of time when all species of any importance will ring the changes to every reasonable extent, though in some of the smaller kinds it would not be easy to detect all the distinctions even if it were possible for them to exist.

DESCRIPTION OF THE CHARACTERS.

Of the marked characters which are now known to run pretty generally through the different species of ferns, that known as "The Plumose" is the most interesting and perhaps the most beautiful. In its nature it corresponds indisputably with the double flower in phanerogamic botany. In this class are to be found some of the most beautiful objects in nature; anything more symmetrically and delicately beautiful than a luxuriant plant of any of the best forms of Athyrium f.-f. plumosum, it is impossible to conceive, nor would it, I think, be easy to find, a much finer object than a well-grown plant of Scolopendrium vulgare crispum of the better sort—(this being the plumose form of that species); and when I mention that Adiantum Farleyense is not a species, but simply the plumose form of A. tenerum Ghiesbrechti, or some other kindred species, I think I shall have said enough. The old, very old, form of Polypodium vulgare Cambricum, is the plumose form of that species. The most marked characters of this class of varieties

TABLE SHOWING HOW CERTAIN CHARACTERS APPEAR IN THE VARIETIES OF DIFFERENT SPECIES OF BRITISH FERNS.

Fixed Characters.	Species.																	
	Polypodium vulg.	Polystichum. ang.	P. aculeatum.	P. lonchitis.	Athyrium f.-fœmina.	Lastrea f.-mas.	L. pseudo-mas.	L. propinqua.	L. montana.	L. dilatata.	Scolopendrium vulg.	Asplenium marinum.	A. lanceolatum.	A. ad.-nigrum.	Osmunda regalis.	Adiantum cap.-Ven.	Pteris aquilina.	Blechnum spicant.
Plumose and Frondose ...	×	×	×	—	×	×	—	—	×	?	×	×	—	?	—	×	—	×
Divided and Decomposite	×	×	×	—	×	×	×	×	×	×	×	×	?	—	—	—	×	×
Imbricate and Crispate	×	×	×	—	×	×	×	×	×	×	×	×	—	—	×	×	×	×
Lax and Flexuose	×	×	—	—	×	×	—	—	×	×	×	×	—	×	—	—	×	×
Deltoid and Brachiate ...	×	×	—	—	×	—	—	—	—	—	×	—	×	—	—	—	—	×
Cruciate	×	×	×	—	×	×	×	—	—	×	—	—	—	—	—	—	—	×
Interrupted and Deficient	×	×	×	—	×	×	×	×	×	×	×	×	×	×	×	×	×	×
Crested and Ramose ...	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×	×

are generally their tendency to a soft and papery texture, a light yellowish colour, and barrenness—especially the latter. It is this that causes the extraordinary beauty in Farleyense;—being barren, the pinnule does not fold over to form an indusium for the sori, and the strength which would, in the ordinary course, be expended in the production of the sori and spores, exhausts itself in additional luxuriance, this foliose character being generally one of the peculiarities of this class.

By way of illustration of the other marked natural characters, let us take Polystichum angulare, one of the four species in which alone all the marked characters have as yet been known to appear; *the* one in which, on the whole, these characters have appeared in the greatest variety (its only possible rival in this respect being Athyrium f.-fœmina), and undeniably the one in which, with the exception perhaps of the cruciate class, they have been developed to the greatest perfection. The natural deviation of P. angulare, indeed, has been such as to render necessary a more complex classification. This, no doubt, is mainly to be attributed to P. angulare being a stipitate, or rather per-stipitate fern, the stipitate character being preserved to its minutest divisions, even to the third division of the lobe or pinnule. This brings us to the next or the Divided and Decomposite Class, which (leaving out the plumose forms, which are also divided) includes the forms with divided lobes or pinnules, a class disputing with the plumose the distinction of producing the most beautiful of the varieties of British ferns. The well known proliferous form of angulare, of which so many improved varieties have since been found and raised, is a specimen of this class.

The Imbricate and Crispate class speaks for itself. These forms are never beyond medium size and are generally more or less dwarf, they are both distinct and pretty. The lax and flexuose forms are just the opposite to the preceding; the lobes, instead of overlapping, are far apart, with a distinct tendency in the frond to assume a weeping habit; the extreme forms of flexuosum being absurdly twisted into knots. In this class are included also those varieties in which either the pinnules or pinnæ are reflexed.

The Deltoid or Brachiate class includes (1) the forms in which the frond broadens abnormally but symmetrically towards the base; and (2) those forms which are trinerved.

The Cruciate class is perhaps the most remarkable of all. In the most characteristic varieties of this class the development of the pinnæ is arrested in a very complete manner at the basal lobes or pinnules, and the whole strength of what should have been the pinnæ being thus thrown into them, they develop to an extent sometimes truly marvellous, assuming almost the proportion and character of pinnæ, and crossing, present the appearance which has suggested the name. In the variety Athyrium. f.-f. Victoriæ the cruciate character is shown even in the minutest divisions. There is a subdivision of this class in which the development of the pinnæ is not arrested, the pinnules only assuming a cruciate character, while retaining their natural size.

The Interrupted class is perhaps the one most generally distributed, and in some of the species, and notably in Polystichum angulare, it is so varied in its extravagances as to require very complicated classification. It includes, as its name sufficiently indicates, all the varieties which are deficient in any respect,—by the partial or total arrest of the growth of the frond, pinna or pinnule. The chief characteristic of this class generally is ugliness, but there are many exceptions in which the deficiency is so symmetrically diffused, that the general appearance of the frond or plant is decidedly graceful, the details being often exceedingly pretty as well as interesting as a study of the vagaries of nature.

The only other class is the Crested or Ramose; this (with the exception of the Brachiate or trinerved forms, which require a place of their own) includes all the varieties in which there is an abnormal repetition of parts. At first sight it might appear that cristation, like variegation, was a character that might run through the varieties of all classes, and no doubt this is true to a very great extent; but there are many varieties in which the reduplication of parts is so pronounced a feature as to obscure all other characters; by general consent, therefore, it has been long recognised as a separate class. There are varieties which, for distinction's sake, I have named "percristate," in which not only is every pinnule crested, but in which even the minuter divisions are crested also. There are, on the other hand, very many cases in which the peculiar character of the frond—plumose, divided, congested, crispate, lax, flexuose, cruciate, or interrupted—is perfectly preserved, and the cresting only super-added; and herein, no doubt, lies a difficulty, but as the difficulties in the opposite direction seem so very much greater, it has been found better to established a "Crested Class."

It is not generally known that a few years since a memorial to the Board of Works was signed by some of the leading British fernists, advocating the formation of a national collection at Kew, and suggesting a money grant in aid of it. The grant was made, and operations were without delay commenced, Mr. E. J. Lowe, Mr. E. F. Fox, and others contributing some of their choicer varieties. The handsome bequest of the whole of his valuable collection of British ferns recently made to Kew by the late Mr. W.

C. Carbonell, of Usk, has come just in time to give a spurt to the undertaking. Hitherto it has been too often the case that collections accumulated with infinite pains and skill, and containing unique things, have on the death of the collectors been dispersed, and thus valuable things have been lost for ever. All honour therefore to Mr. Carbonell for showing how to prevent this. Nor can it be doubted that when it is generally known that a permanent home for ferns exists, in which any rare thing will be safe and at the same time of public use, finders and raisers will feel a pride in giving of their best, feeling sure that, under the present progressive and discriminating management at Kew, anything really good will be appreciated, and that nothing accepted will be neglected.

The time has happily passed when all botanists were content to regard the varieties of ferns as only "monsters," mere "garden varieties," etc. It has long been recognised that, with very few exceptions, all the more marked forms have been found wild, Nature being entirely responsible for them. It is also beginning to be recognised that varieties do not appear altogether at haphazard, but in conformity with certain fixed laws of development or deviation to which all species are more or less subject.

The varieties of ferns therefore cannot any longer be dismissed without attention, even if the extreme beauty of many of them did not render this impossible. He must indeed be wanting in some quality of sense or in knowledge who can regard all the varieties of British ferns only as degradations. Can it be maintained that many of them are not in every sense higher developments, possessing with all the symmetry of the normal form greater delicacy of division and of texture, more freshness and variety of colour, more grace of habit, often larger size, and at times an intricacy of detail or novelty of structure which interests the mind not less than it attracts the eye?

Such a collection as it may be hoped will now be formed at Kew, containing all that is most beautiful, rare, and strange amongst the varieties of British ferns, would not only be an additional attraction to any garden, but in the not uninteresting study of the morphology of plants a very practical aid. Nor should such a collection fail to excite a special interest in this country as being illustrative of, and at the same time a record of a branch of botany exclusively British, and likely to remain pre-eminently so; for whatever discoveries in other parts of the world may be in store for the future, it is at present the fact that in no country of any considerable size has the natural tendency of ferns to vary been developed to anything like the same extent as in the British Isles.

It has been well remarked that it would seem as if Nature had compensated for the small number of species she has allowed us by gifting them with an unlimited power of deviation. No doubt this power varies very much in different districts. No one who has really given attention to the subject can have failed to recognise how marked are the distinctions in this respect in our own country, some districts being hopelessly barren of any varieties of note, while other parts more favoured positively teem with them. How far this may be due to soil, how far to climate, or to a combination of the two, or to any other material agency, or whether the fairies may not have a word or two to say about the matter, I leave to others to determine. The fact however cannot be disputed, and, being so, why should not the principle have larger application? Let us therefore, while we can, enjoy the idea that in this respect we are a favoured country.

NOTES SUPPLEMENTAL TO THE

Flora of the Bristol Coal-field.

1887.

BY JAMES WALTER WHITE.

Draba brachycarpa, *Jord.* *Erophila præcox,* Reich.

Little has been done locally, or indeed in Britain, towards separating the seventy forms of *Draba verna* described by M. Jordan. Many of them are probably confined to the Continent. Three only have places in the last edition of the London Catalogue, namely, the type, a Scotch plant from Ben Lawers, with inflated capsules, and *E. præcox.* The latter is a form with broad, rounded pods, which has been observed for several years on walls near Stone Easton, S.

Sisymbrium pannonicum, *Jacq.*

An alien crucifer. It was growing luxuriantly at St. Philip's Marsh, G., in June, 1887.

Lepigonum salinum, *Fries.*

Native. In fair quantity on sea-banks and muddy shores in the Channel about Burnham and Clevedon, S.

Saxifraga granulata, *L.*

This has been found by Mr. D. Fry during the past summer at Stanton Drew, and also in the neighbourhood of Chew Magna; at the latter locality in considerable abundance.

Previously the only station at which it was known to exist, either in the Bristol district or in the whole of Somersetshire, was a spot near the village of Stanton Prior (see Fl., p. 74); and it is interesting to find that the distribution of this plant in N. Somerset is wider that was hitherto suspected.

Taraxacum officinale, *Web.* var. *β. erythrospermum*, Andrz.

This well-marked variety, which had not previously been observed or distinguished, was found during the summer on wall tops, at Stanton Drew, S., and Brandon Hill, G., by Mr. David Fry.

It is clearly separated from *Taraxacum officinale* proper (var. *a. Dens-leonis*, Desf.) by its dwarf habit, more deeply pinnatifid leaves, and also by the colour of the fruit, which, when ripe, is a dark, glossy, brick-red, not olive or dull yellow, as in the typical plant and all the other varieties.

Cuscuta europæa, *L.*

Additional records. Towards the close of a July ramble over the Wilts border that had extended as far as the famous South Wraxall bogs, where we had gladly seen *Lysimachia thyrsiflora* by no means "on its last legs," as had lately been reported, my companion, Mr. A. E. Burr, pointed out as we returned by way of Bathford, a good patch of this dodder, by the river bank, growing on *Sinapis nigra*, *Conium*, and *Galium*. Mr. Burr had found it also on the other side of the Avon, below Bathford, and says that it occurs in several places not far apart. He remarks that it grows chiefly on *Urtica dioica*, but also on a great many other plants besides those mentioned, namely, *Symphytum*, *Nepeta*, *Achillea*, *Sparganium*, *Scrophularia*, and *Epilobium hirsutum*. Mr. Burr's stations are just outside our area. However, a week or two later, specimens on the large nettle were brought to me by Mr. J. C. House, who also had gathered them on the river bank, but some miles lower down, at Hanham, G. It is probable, therefore, that the Cuscuta occurs at intervals in the Avon valley, and may have a fairly extensive range.

Mentha. Note communicated by Mr. D. Fry.

Since the publication in 1883 of Part III. of the "Flora," containing the Labiatæ, some important additions to our records referring to the genus Mentha have been made, by the discovery of the following species, at the localities noted below.

Mentha Piperita, Huds. S. Clevedon. Woollard. Worle Hill.

The Clevedon plant is var. *β. vulgaris*, Sole: that from Woollard appears intermediate between var. *a. officinalis*, Hull, and var. *β*, having the elongated leaves of the former variety with the capitate spikes of the latter. The plant from Worle Hill presents some marked divergences from either of the two varieties of *M. Piperita*, Huds., as at present recognised in the British flora; and as several leading botanists, who have seen specimens, are unable to say under what variety they should be placed, we can only refer them to *M. Piperita*, Huds., as an aggregate species.

The Clevedon station has probably been destroyed by recent alterations in the ground connected with local improvements, which is to be regretted, as the variety found there is a rare and interesting one.

Mentha rubra, Sm. This handsome Mint has been found both in the Gloucestershire and Somersetshire divisions of the district, at the following localities:—

G. Crew's Hole. Conham.
S. Bank of Avon, under Leigh Woods.
Congresbury. Litton.

Scilla autumnalis, *L.*

It is gratifying to be able to announce that the hope expressed in the "Flora" (p. 201), that this rare bulb might yet be re-discovered on St. Vincent's Rocks, has been justified. We are indebted for this pleasure to Mr. J. C. House, who, during a scramble in the autumn, came upon a patch of about a hundred plants. It was somewhat perplexing, however, to find that the spot was *made* ground, the site of ancient quarrying; but this circumstance has been explained and accounted for in a very interesting and satisfactory manner. Mrs. Glennie Smith has kindly furnished information on the matter that was conveyed to her by Mrs. Glennie, widow of Mr. William Glennie, who was engineer, under Brunel, of many great works in the West of England. The account runs as follows: When Brunel was about to commence the construction of the Suspension Bridge, Mrs. Glennie told him that he was going to destroy the Clifton locality of *Scilla autumnalis*, as it grew just where the approach on the Gloucestershire side was to be made. The engineer immediately informed himself carefully of the exact spot, and before the ground was broken he made some of his workmen dig up the turfs containing the bulbs, and transplant them safely beyond the reach and influence of the works he was about to begin. Mrs. Glennie could not remember if she ever knew the place to which the transference was made; but it seems tolerably clear that Mr. Brunel's care was effectual in preserving for us a choice plant, the locality for which, when undisturbed, was evidently of very small dimensions.

Juncus bufonius, *L.*, var. *fasciculatus*, Koch.

A well-marked variety, characterized by its short thick stem and fascicled flowers.

S. Road-sides near Norton Malreward. *Mr. D. Fry.*

Juncus Gerardi, *Lois.*

A pretty little rush, allied to this species, has for some years been under observation. It grows plentifully in a brackish marsh on sand by the Channel shore near Berrow, between Brean and Burnham; and its interest depends on characters linking it with *J. compressus*, Jacq. This summer (1887) I have been enabled to study the latter plant from specimens obtained near Stanton Drew, and having also gathered typical *Gerardi* on the coast of Dorset, could determine the position of the Berrow rush with some confidence. *J. Gerardi* is a salt-marsh plant, distinguished by a far-creeping rhizome, panicle rather close, exceeding its bract, and capsule narrow, strongly mucronate, about equalling the perianth. On the other hand, *J. compressus* is found only inland, has a tufted rhizome, a rather loose panicle falling short of its bract, and differs above all in the larger, rounder, and more obtuse capsule which distinctly exceeds the perianth.

The plant under notice has the rhizome of *Gerardi*, and, unless hampered by other vegetation, creeps straight ahead in a direct line, putting up stems at regular remote intervals. It agrees with that species also in the comparative length of the lower bract. There the similarity ends; the panicle is loose, with separately stalked flowers; the perianth segments fall short of the capsule, sometimes by as much as one-half; the capsule is never acuminate, but subglobular, obtuse, and mucronate, of a beautiful light-brown colour, polished and shining when fresh, becoming puckered and wrinkled on drying. Dr. Buchenau, the chief authority on Juncus, reports on specimens sent to him: "Forma intermedia *J. compressi* et *J. Gerardi*. Antheræ filamentis circa 2½-plo longiores. Stilus longus. Fructus perigonio circa dimidio longiores."

The Berrow rush, therefore, is a connecting link between the two species mentioned; and although such a form is extremely rare, and perhaps may now have been observed in Britain for the first time, yet its occurrence decidedly supports the view of those botanists who consider these plants to be resolvable into one super-species through intermediate states.

Juncus diffusus, *Hoppe.*

New to Somerset. A specimen of this little-understood, and apparently very rare plant, gathered at Dean, near Cranmore, has been received from the Rev. R. P. Murray, who says that it was considered *diffusus* by Mr. T. R. Archer Briggs.

Calamagrostis lanceolata, *Roth.*

An addition to the "Flora." On a recent visit to Mr. T. B. Flower, that gentleman showed me, in his herbarium, specimens of this grass, labelled in Mr. Thwaites' hand-writing, "Arundo Calamagrostis, *Linn.* Filton Meads, Gloucestershire, Aug., 1846. *G. H. K. Thwaites.*" It is probable that, if carefully searched for, the plant will still be found existing at the locality given on the label.

The Mendips: A Geological Reverie.

By C. LLOYD MORGAN.

I HAD mounted the broad back of Mendip. Hammering all day, and many a day before, at the geological deposits which fringe its margin, I had constantly endeavoured to picture to myself the old-world times of which they spoke, when this range of hills stood out as an island in the midst of a warm sea, teeming with those strange and unaccustomed forms of life which the geologist disentombs from their stony graves in the secondary rocks. Quaint reptiles, the swan-necked Plesiosaur, and the frog-necked Ichthyosaur, sported at the surface or sought their prey in the depths below. Queer fishes, some clad in scales like shining armour (ganoids), others the distant ancestral relatives of the Port Jackson shark of Australian waters, swam lazily hither and thither, or darted away before some hungry cone-toothed saurian. Strange shell-fish, the Ammonites and their tribes, the abundant lamp-shells, and less unfamiliar bivalves, might be seen through the clear water, together with sea-urchins, and corals, and waving sea-lilies. On the island itself ferns, cycads, and coniferous trees may have given to the vegetation an Australian aspect. In the streamlets were stonewort and fresh-water molluscs. Small pouch-bearing mammals, first of their kind, timidly hunted their insect prey (beetles, and may-flies, and grasshoppers), little dreaming of the future predominance of their race. If birds there were, they hid themselves from view; but in the air were leathern-winged reptiles, with cruel toothed jaws. Great slouching land reptiles may have been the lords of the Mendip land of old.

Some such picture had been before my mind's eye as I worked my way up Vallis Vale and Whatley Combe, past Nunney Castle and the Holwell quarries, to the central ridge of Eastern Mendip. And then, having reached the old red sandstone upland, I threw aside my geological hammer, and lay me down on the short, sweet grass which clothed the slope of a British round barrow, to enjoy the fair view and the pleasant southerly breeze upon my brow.

Near me, to the east, lay Beacon Hill, on the wooded summit of which, the highest point of Eastern Mendip (1,050 feet), the contrasted green of larch, beech, and pine showed that spring had not yet fully ripened into summer. To the west I could just see the trees which mark from a distance the site of Maesbury ring, a fine earthwork defended by a double rampart. To the south-west Glastonbury Tor, backed by the dim blue line of the Quantocks, formed a feature in the landscape that could not be mistaken; while nearer Wells were the mountain limestone hills, which must have stood out as islets in the warm mesozoic sea over the deposits of which my eye ranged. To the south-east Alfred's tower at Stourhead marked the incoming of strata (upper greensand) laid down in a later secondary sea.

My thoughts still continued to run in the same channel, and I mused on the vicissitudes which the country around me had undergone. Long before the Mendips had any existence as a range of hills, the Old Red Sandstone rocks which to-day form their heart, were deposited near the margin of a sea which lay over what is now Devon and Cornwall, spreading thence eastwards through northern France and Belgium. Their deposition marked the close of a long era, the Devonian of geologists, during which the northern parts of our islands lay within the coast-line of a great continent stretching away northwards and perhaps westwards, nobody knows how far. In the early part of this period there lay within the bounds of this continent a series of great Scottish lakes, tenanted by strange types of old-world creatures. Of the armoured fishes, represented to-day by such scattered forms as the Polypterus of the Nile, the Calamoichthys of Old Calabar, and the Alligator-gar of the American lakes, there were many and various representatives, some with sharp, thornlike fin-spines; others with enamelled, bony helmet; and many with overlapping smooth or wrinkled scales. With them were the quaint Eurypterids, welcomed by the author of the "Vestiges" as crustaceans, struggling not unsuccessfully to become fishes; and welcomed again by pious quarrymen, who regarded their ventral plates, or fused appendages, as the fossil wings of cherubim.

One of the Scotch lakes in which these creatures lived, Lake Orcadie (see Map 1 *), lay to the north of the Scottish Highlands, reaching to the Orkney and Shetland Isles,

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* The Scotch lakes were named and defined by Dr. A. Geikie. Mr. Jukes-Browne, however, questions the distinctness of all these basins. The map is modified from that given by Prof. Hull in his "Physical History of the British Isles." The reader is, however, *earnestly warned* in regard to this and succeeding maps, that the boundaries laid down in them cannot be fixed with any approach to certainty. They are aids to the imagination, and serve to give the history of the past an air of reality; but they are constantly liable to modification by further and

and perhaps extending to the present coast of Norway. Another, Lake Caledonia, occupied the central valley of Scotland, and spread thence to the north of Ireland. Smaller lakes occupied the site of the Cheviots and of northern Argyllshire.

Not only were there great lakes in Scotland at the time of which I speak, but this region was also one wherein vast volcanoes poured forth immense streams of lava, which flooded the country and were interbedded with the sandstone and other deposits that were being swept piecemeal by rivers and streams into the basins of the Scottish lakes. By these deposits, volcanic and other, the lakes were to a large extent silted up. But it would seem that elevation of the country also took place, and the lakes may have been drained of their waters by the deepening of the valleys through which their effluent rivers flowed seawards. The country would then have presented "the aspect of a high and dry upland formed of lofty hill-ranges, separated by immense plains, the sites of the desiccated lakes." But this state of things was not to last. "A reverse movement at length set in toward the end of the Devonian period; portions of the old lake-basins were again filled with water, the area of which widened and deepened as the land sank; torrents washed in the detritus of the land, and the material thus collected became the conglomerates and sandstones of the upper old red and lower carboniferous series" (Jukes-Browne).

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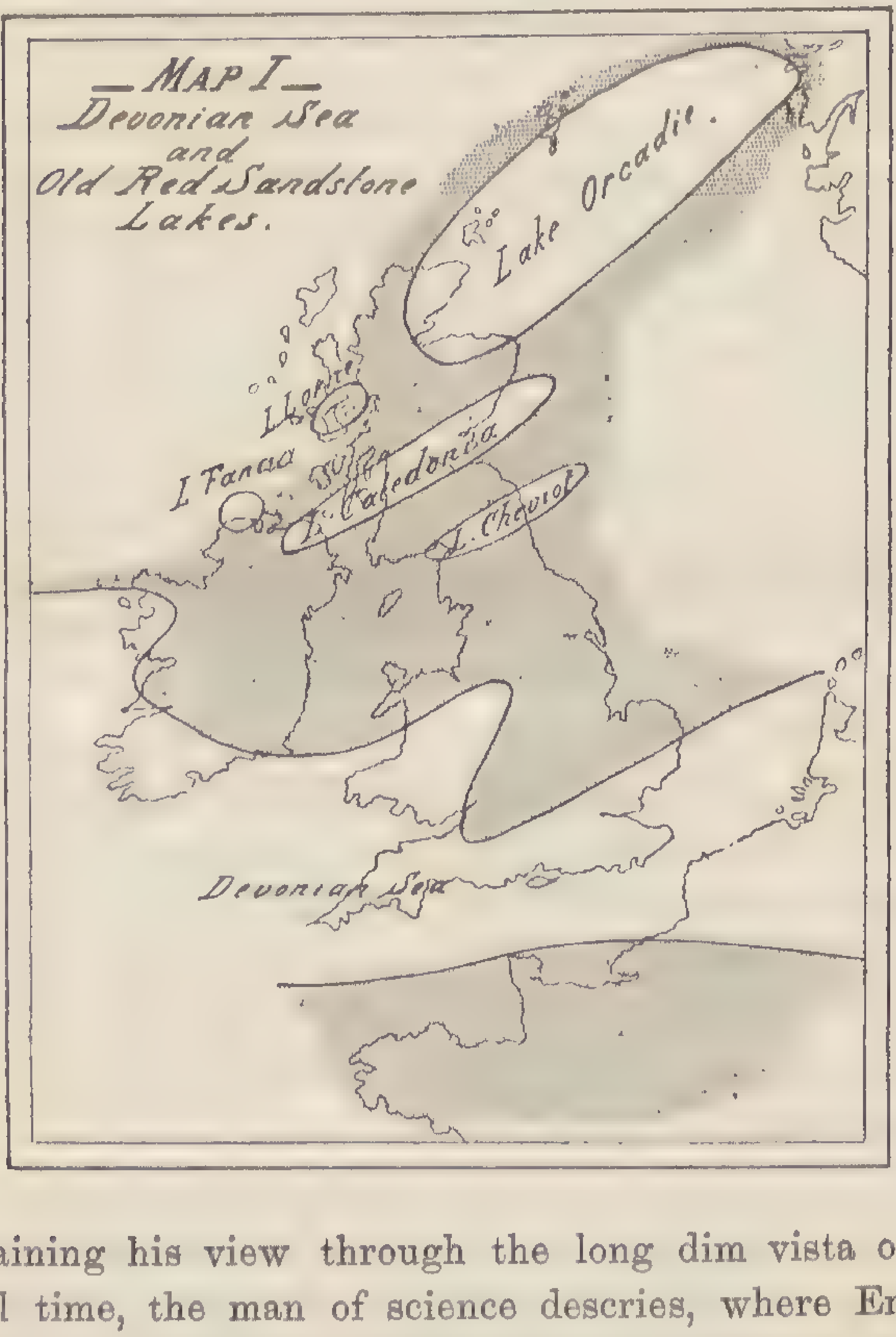
fuller investigation. In the preparation of the succeeding maps, with the exception of that of the Mendip Isle, for which I alone am responsible, I have been aided by Mr. A. J. Jukes-Browne, whose forthcoming volume on "The Building of the British Isles," some of the proof-sheets of which he has, with great courtesy and kindness, allowed me to see, will form a most interesting and valuable contribution to the palæogeography of our islands.

Such was perhaps the sequence of events in the northern part of our area during the period at the close of which the Old Red Sandstone of the Mendips was deposited.

Let me devote one paragraph to the history of preceding events before continuing the story of the Mendip area.

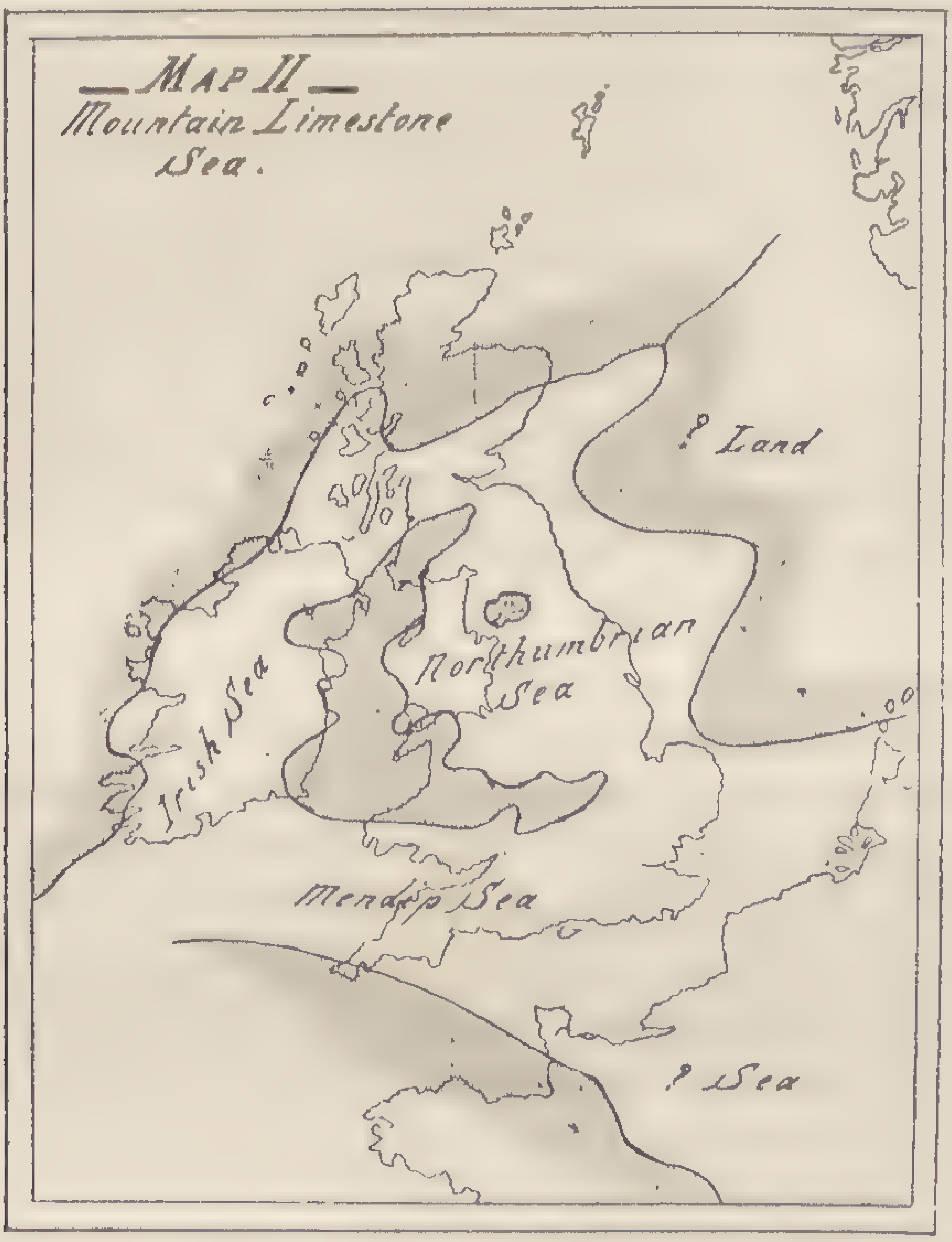


Straining his view through the long dim vista of geological time, the man of science descries, where England now is, and stretching northward and westward, a great Archæan continent. How the rocks which composed this continent were formed we know not for certain, so much alteration have they undergone; but it would seem that they largely resulted from the outpourings of volcanoes. This is the earliest land of which we have cognisance. Rain beat upon it; the sun shone down upon it; rivers ran over its surface; the waves of the sea dashed against its coast-line; giant tides swept its seaward margin. Nor was it more stable than the continents of to-day. South Greenland to-day is gradually being submerged; the shores of the northern Baltic are being slowly lifted out of the water. The Archæan continent, I say, was not more stable, and gradually subsided beneath the waters of the great Silurian ocean. On the bed of this ocean the *débris* torn from the ancient continent by rain, by rivers, and by the waves of the sea, came to rest, and were built up into the strata, thousands of feet in thickness (Cambrian, Ordovician, and Silurian), out of which the mountains of Wales have since been fashioned and carved. From time to time islands appeared in the Silurian sea, but were submerged or washed away. For long ages did this sea roll over the site of our English homesteads. And then elevation of the sea-bottom took place. Scotland became part of the great northwestern continent, and bore upon its bosom the broad lakes of which I have spoken; and the waters of the ocean were forced to retreat southwards, till of all England only Devon and Cornwall remained submerged, though an arm or inlet passed upwards into South Wales and occupied the area of the Mendips. In that arm of the southern Devonian sea was the Old Red Sandstone, which constitutes the heart of Mendip, formed.

Geology, which in one of its aspects is the physical geography of the past, is the history of continued change; and the Mendip area was destined soon to be more deeply submerged beneath a clearer sea teeming with ancient forms of life—sea-lilies, lamp-shells, and branching palæozoic corals. The skeletal remains of these creatures were built up into thick masses of limestone which, since it now forms hill-ridges, the main mass of the Mendips among the number, was called by the older geologists the Mountain Limestone. The sea wherein it was formed was not improbably a land-locked mediterranean. Its northern shore perhaps ran through the Highlands and along the north-west of Ireland. Its southern shore probably ran through the north of France, but perhaps touched Cornwall. Eastwards the sea stretched through north-east France, Belgium, Germany, and Poland, into Russia. Westward it may have opened by narrow straits into the greater ocean.

We have evidence from the thinning of the strata and the occurrence of beach-conglomerates that, within this Mediterranean Sea, there was land extending from Mid-Wales into Leicestershire; there was land, too, north of Wexford and Waterford in the Wicklow hills of Ireland; there was land in County Down; there was land in the southern uplands of Scotland. Mr. Jukes-Browne, who has ably marshalled the evidence, includes these land areas in the large island, the position of which is indicated in the map (Map 2). Professor Hull gives a different reading, extending the land of Mid-Wales and Leicester eastwards, as a "central barrier," to join the continental land in that direction, and marking the Wicklow, County Down, and Southern Upland land-regions as isolated islands; while Prof. Green extends the central barrier westwards to join a land area between Ireland and France. In any case it seems very probable that within the Lower Carboniferous Mediterranean, the southern Mendip Sea was partially separated by an island or ridge from the Northumbrian Sea to the north, in which, at the beginning of the period, the

Cumbrian Lake District rose as a small island. And in the Mendip Sea the waters were clear and bright but not very deep to the north; to the south, however, over Devon they were turbid and muddy owing to the clay and silt brought down by the rivers that drained the continental land lying in this direction.



Long ages again rolled by and these seas became shallowed through the unceasing deposition of silt and sand near the river mouths, and, in the clearer water, by the accumulation of the remains of the abundant crinoids, corals, lamp-shells, and other marine creatures; and this, notwithstanding that the sea-bed was undergoing steady, though it may be unequal subsidence. Then, in course of time, the waters of the northern part of the Mendip Sea became more turbid, and sandstones and shaley beds were interstratified with the limestones; for we must remember that rain and the weather, aided perhaps in the uplands by giant frost, were doing their worst upon the bordering continental land, and great rivers were washing down the resulting *débris* into the land-locked seas. Thus by the deposition of limestones, shales, and the millstone grit the Mendip Sea was silted up, and throughout the mediterranean area there was formed a series of enormous swamps, on which there sprang up a rich and luxuriant vegetation of ferns and giant club-mosses, and tall fluted reeds, among which great stupid, shovel-headed, salamander-like labyrinthodonts "pottered, like Falstaff in his old age, with much belly and little leg." From time to time submergences took place, like that which in 1811–12 converted a huge area of the Mississippi delta into a vast sunk country. But ere long the submerged tracts were again silted up; the rank vegetation obtained once more a foothold; and fresh accumulations of peaty and other vegetable matter were formed, only to be again, by reiterated submergence, more and more deeply buried beneath silt and sand, and thus to be stored as coal for the future use of man.

How long this state of things continued it is impossible to say. But we know that there was time enough for the coal measures in South Wales to attain a thickness of from ten to twelve thousand feet with seventy-five seams of coal. It was a period when geographical changes were slight, when terrestrial disturbances were at a minimum, and when the denizens of sea and land, of marsh and swamp, found everywhere a similarity of conditions that enabled them to flourish and survive without change or variation. For, as Mr. Jukes-Browne well remarks, "when the seas were shallowed by the continued deposition of material derived from the continents, and when the higher land was everywhere encircled by a wide belt of low-lying jungle and swampy ground, intersected by sluggish waterways and lagoons, the conditions would be exactly those where nature would present a monotonous and uniform aspect, and where the plants and animals which had established themselves would be likely to maintain their existence unchanged so long as the same conditions prevailed."

But this period of quiescence, protracted as it was, could not last for ever. It was succeeded by an epoch of severe earth-throes. Pressure set in both from the north and south, and from the east and west; and the strata that had accumulated under conditions so peaceful were squeezed between the jaws of the surrounding continental lands. By the gradual and resistless pressure thus brought to bear upon them the horizontal deposits were thrown into ridges, some running east and west, some north and south, just as a piece of cloth puckers up in ridges when the ends are pressed together. One of the most marked of the east and west ridges is that of the Mendip Hills, which then had their birth after their prolonged period of gestation in the womb of the carboniferous seas. And perhaps a great igneous dyke which pierces the Old Red Sandstone near Stoke Lane may have been formed at the period of this upheaval. Of the north and south ridges, the Pennine chain, sometimes called the backbone of England, is one of the most conspicuous. In the basin-shaped or oval troughs between the ridges, the coal measures were preserved from that destructive denudation wrought by rain and frost, by rivers and ocean waves, which has removed vast piles of rock from the summits of the ridges themselves.

Many geologists believe that it was during this period of land upheaval, and the earth-ridging of our north-European area (the Alleghanies of America being formed about the same time) that the North Atlantic Ocean had its birth, by the subsidence of an ancient continent of Atlantis. Be this as it may, there is little doubt that some of the main geographical lines of the Western Europe of to-day were now beginning to be sketched out.

Could we have stood on the newly formed Mendip ridge, we should have looked out on a great continent stretching eastward through France. Around us we might have seen a vegetation in some respects allied to that of carboniferous times; but the coniferous trees, which were restricted to the uplands of those days, would preponderate over the reeds and ferns and gigantic club-mosses of the low-lying coal-measure swamps. The great mediterranean sea had now shrunk into restricted Permian lakes, lying on either side of the developing Pennine axis, tenanted by the stunted descendants of the marine creatures which had lived in the waters of the Carboniferous seas.

Again long ages passed by. The continent was perhaps further upheaved and the great lakes were drained dry; or their level may have been gradually reduced, and their waters rendered saline by the accumulation of mineral salts through the long prevalence of a rainless condition of the atmosphere.

The curtain next rises on a new act in the geological drama. The successive scenes of the Palæozoic act are played out. Those of Mesozoic times now begin. And as the curtain rises with the epoch of the Trias the Mendips are still seen as a hill range in an almost desert continent.

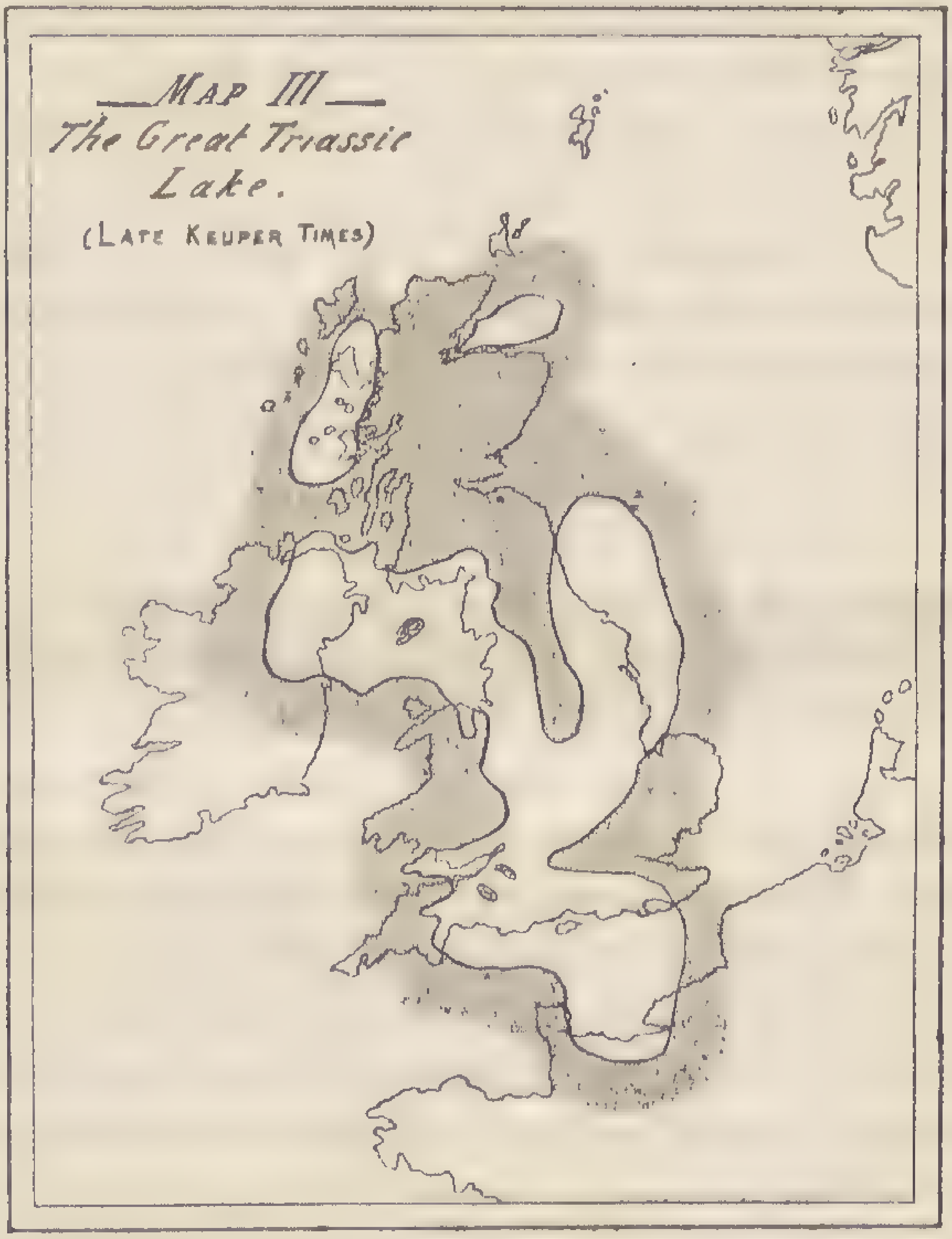
To the south of them, over parts of Devon and Dorset, lies a Bunter Lake, girt to the west and south by a rocky and iron-bound coast, stretching from Cornwall into the north of France.

To the north, over the sites of the Permian lakes on either side of the Pennine axis, the occasional torrential rains of winter swept down from the valleys pebbly and sandy detritus and spread them over the plains, which, during the greater part of the year, may have "formed bare and arid deserts, over which hot winds whirled clouds of sand, and on which no living creature could find sustenance." In this way, according to Prof. Bonney, were the pebble-beds and wind-blown sand-stones of the Bunter formed; but it is not improbable that during periods of exceptional rainfall the arid plains may have been converted into temporary shallow lakes.

In any case, with the advent of later Triassic (Keuper) times, through changes of climate and slow subsidence of the land, the areas of these Triassic lakes enlarged, the waters on either side of the Pennine axis broadened and deepened, and extended southwards, while those of the southern lake over east Devon and Dorset crept further and further northwards until the northern and the southern lakes became merged in one sheet of water, and the Mendips became first a promontory of the western land, and then an isolated island at the mouth of a gulf which separated Devon from South Wales (Map 3).

Time had, however, wrought its ravages on the Mendips, the main scenic features of which, as we now see them, having, as I believe, been then impressed upon them.

Let us turn aside for a moment to consider the part played by this old denudation in producing the Mendip Hills as we now know them. After the deposition of the strata formed in the old coal-measure swamps, an east and west fold (anticlinal axis) was formed through the flexing of the rocks due to lateral pressure setting in from north and south. That axial ridge was to the Mendips of to-day what the block of Carrara marble, as it comes to the hands of the sculptor, is to the finished product of his art. It was merely the rough material out of which the hills should be fashioned. We must not suppose that the process of upheaval was a sudden or cataclysmal one. It was one which probably took ages to accomplish. The recently-formed beds, too,

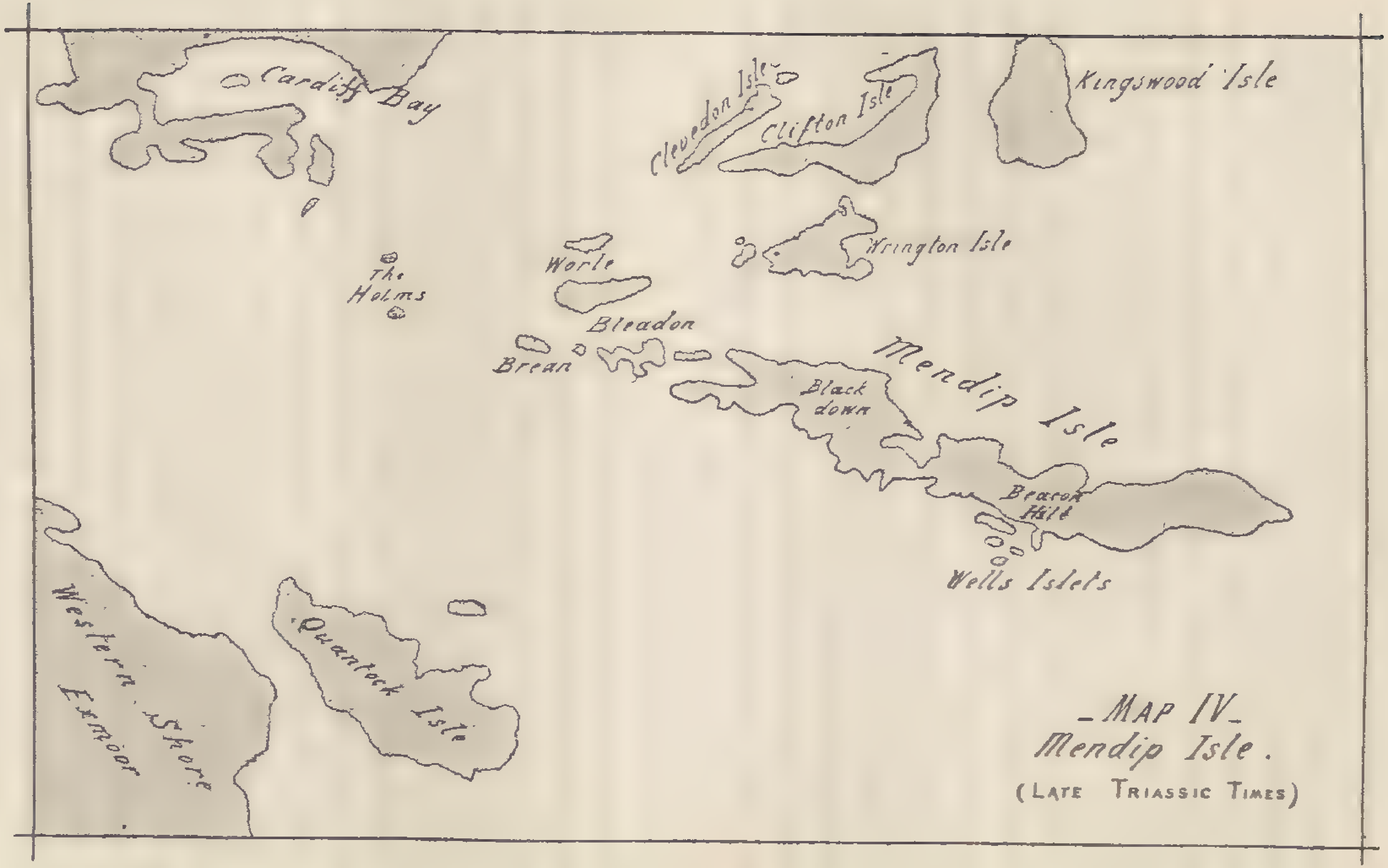


were not then so hard as we now find them to be, and had not been consolidated into sandstones, grits, and crystalline limestones. And as they rose step by step they were subjected to the ceaseless action of the forces of denudation. The waves of the sea cannot have played much part in this denudation; it must have been in the main sub-aerial.

Two processes of sub-aerial denudation are now going on over the surface of the British Isles, a superficial scouring of the whole area by the action of rain and frost and atmospheric disintegration, and a linear trenching of the land along the special lines of river drainage. The surface so acted on is composed of rocks of differing hardness and powers of resistance to denuding processes; and according to their method of formation and method of upheaval these rocks are arranged with a greater or less amount of symmetry. In some places the harder and softer bands alternate with some regularity, and the symmetry is of a comparatively simple type. This, for example, is the case in the neighbourhood of Bristol and to a large extent in the Mendips. But in others, the Highlands of Scotland for instance, the symmetry is of a far more complex type. In any case the essential fact to grasp is this: that whereas the linear trench formed by a river is down-cut to the same depth in hard and soft rocks, the superficial scouring is far more effective on the less resistant than on the more resistant strata. While the softer rocks are being uniformly lowered by general denudation, the harder strata will become fretted with deep ravines and gorges through the linear denudation of the streams and rivers.

Applying these general principles to our special case, we see in the Mendip Hills the nuclear heart laid bare to our view by the scalpel of denudation by which the overlying beds have been removed. The range that we see is only the central core of the ridge-fold which was formed at the close of Palæozoic times. This core better resisted the action of that ceaseless and insidious sub-aerial denudation which, during the long continental conditions of Permian and early Triassic times, was lowering the surface of the surrounding coal-measures that lay in the troughs on either side of the ridge. And I am inclined to believe that many of the most striking Mendip valleys and gorges resulted from the fretful file-like action of torrential rivers which leapt down the flanks of Mendip at a time when that range formed the uplands of a desert continent visited at times during the winter months by occasional storms and deluges of rain. Be that as it may, there can be little doubt that not less than 11,000 feet of rock had been removed from the summit of the Mendips before they again sank beneath the waters.

Let us try and picture the view from Mendip over the deep blue waters of the broadening Triassic lake. An intelligent microlestes standing on Blackdown (Map 4), then much higher than now, would have seen the salt waters of the lake encompassing the island on all sides. Away to the S.W. he would have seen another large island, the Quantock Isle, and beyond that the western shore line of the lake rising into high ground in Exmoor. To the N.W. the shore line would again be clearly visible passing along the rising land of Wales and Monmouthshire, and trending northwards till lost to view. Between Exmoor and Wales his eye would have followed an inlet narrowing westwards (instead of, as now, eastwards) along the Severn sea. The main Mendip Isle on which he stood must have ended westward in two promontories,—a deep bay running in between,—the more southerly of which, now Wavering Down, was almost isolated from the main island. Further westward, Banwell, Bleadon, Brean, and Worle stood out as separate islets. Eastwards



– MAP IV –
Mendip Isle.
(LATE TRIASSIC TIMES)

the Mendip Isle stretched further, in all probability, than the present extension of the range of hills. Northwards our microlestes would have seen the low island of Wrington, and beyond it the long Clevedon and Clifton Isles. Probably Kingswood formed a coal-measure island east of the present site of Bristol. Such may have been the outlook from Blackdown of old.

Slowly the Mendip Island sank. Slowly the waters of the inland sea crept up its flanks, receiving there certain longshore deposits visible to this day. But before the hills were submerged a change had come over the waters of the sea. No longer, as heretofore, salt and barren of life, they teemed with marine creatures. Subsidence of land over France had placed the waters of the inland lake in communication with the warm Jurassic sea that had long rolled over the region in which the Alps were subsequently to be upheaved as a magnificent mountain range.

One of the most interesting chapters of Mendip geology is that which deals with the gradual submersion of the island beneath the Triassic and Liassic sea. The earliest deposit which speaks to us of this submergence is the so-called Dolomitic Conglomerate. It contains, cemented into a giant's "pudding stone" of often considerable hardness, huge irregular fragments of the local rocks that had perhaps lain at the surface during the long period of sub-aerial waste and decay. I have at times been tempted to believe that some of the huger blocks must have been ice-borne, but I have no evidence that such was the case. And it is perhaps more probable that they were swept from the hills by floods.

While this deposit was being formed along the shore lines of the ancient islands, Keuper marls were being laid down at a little distance from the margin. But whereas the Keuper Deposits of Cheshire are some 3,000 feet in thickness, they only attain a thickness of 200 or 300 feet in the Bristol* or Mendip area. And this I take to be evidence of the fact that this area was not submerged till near the close of Triassic times.

Then came the irruption of the sea. At first only a few bivalves found their way into our area. The bone bed at the base of the Penarth series does indeed include a great number of teeth, bones, and scales of fishes and other vertebrates, including the teeth of the Ceratodus, a fish closely allied to the Flathead of Queensland waters. But these were the unfortunates who succumbed to the changed conditions of environment. The immigrants who made their home in the lake, which was now in connection with the southern Rhœtic Sea, were restricted to a few bivalves. They were soon followed, however, by the teeming wealth of Liassic times, and the abundant fauna of the succeeding Oolites.

Those who are acquainted with the Lias only in its typical development, at Lyme Regis or Whitby, for example, might well be puzzled at first sight of the Lias on Mendip. It is here a thin and meagre shore deposit, with none of the characteristic Ammonites so familiar to collectors. In some places, as at Shepton Mallet, the Lias rests directly on the worn and upturned edges of the Palæozoic rocks, and consists of a close, white limestone; in others, as at Harptree and Emberrow, it has been converted into a silicious chert. Elsewhere, as at Holwell, Rhœtic and Liassic remains have been found in so-called dykes, which must have been fissures at the time when the shallow waters of the Mesozoic sea played over the margin of Mendip Isle. At what exact period the fissures were thus filled in it is difficult to say. Most of them contain Liassic shells; but Charles Moore found in his "microlestes quarry" great numbers of Rhætic forms, scales and teeth of fishes such as *Sargodon*, *Lophodus*, *Hybodus*, and *Saurichthys*, and teeth of the marsupial mammal *Microlestes*. These may, however, have been washed in during later Liassic times. In the Charterhouse mine he found, commingled with marine shells, the freshwater *Planorbis* and terrestrial molluscs, including snails and a chrysalis shell. Everything points to marginal deposits in close proximity to a land surface. And whereas in parts of Gloucestershire, north of the Mendip axis, the Liassic strata reach a thickness of something like a thousand feet, and in Dorsetshire, south of the axis, a thickness of eight or nine hundred feet, on the flanks of Mendip Isle Charles Moore found the whole series to be represented by some thirty feet of deposit. Elsewhere, as in Vallis Vale, the Lias may be wholly absent, beds of Inferior Oolite resting directly on the Mountain Limestone, which is pierced by boring shells, and has old-world oysters still adherent to the ancient rock-surface. The lower Oolites tell somewhat the same tale as the Lias; for whereas they are some 700 feet thick in Dorset, and nearly 500 near Cheltenham, near Frome they do not attain a greater thickness than one hundred feet. Mr. Jukes-Brown thinks it not unlikely that there was an eastward extension of the Mendip axis forming a submarine ridge separating the Dorset basin from that of Gloucestershire.

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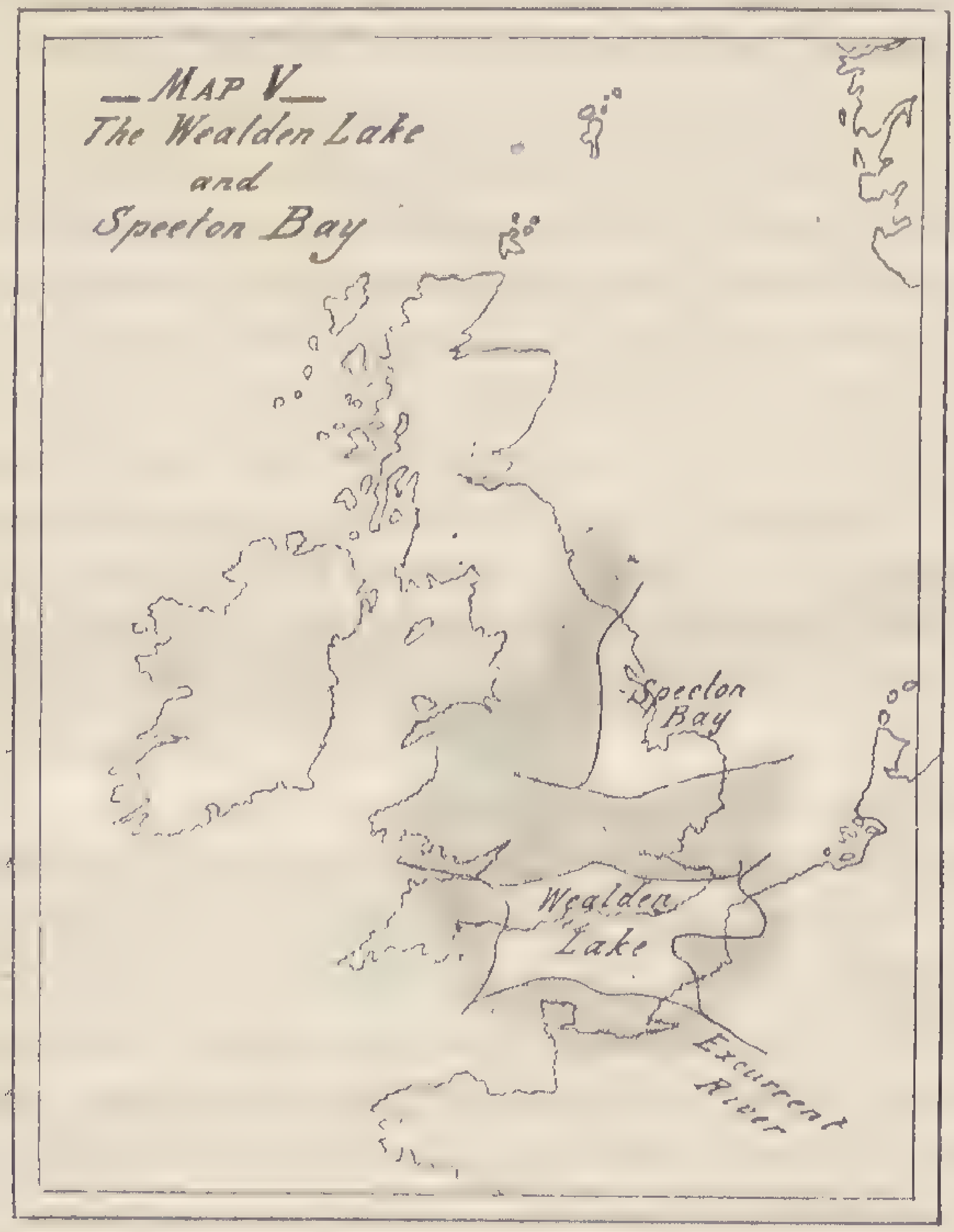
* There would seem to be a local unconformability in the Trias displayed in the new G. W. R. cutting near Brislington.

In the waters of the sea around the sinking Mendip Isle there were deposited beds of clay, with a rich fauna containing Ammonites in abundance, and masses of limestone, due to the growth and waste of coral-reefs, and the accumulation of the remains of shell-fish, sea-urchins, and branched sea-lilies. By such deposits were the seas around Mendip silted up; and when the island itself was finally submerged (perhaps at the time of the Oxford Clay) beneath such deposits was it buried.

There is indeed no definite evidence of the final submergence of the island; and it may be that, as Charles Moore was inclined to believe, it was not submerged at all in Jurassic times. The Oxford Clay is, however, a formation of wide extent, and of comparatively uniform thickness and character. Undoubtedly it was formed in deeper water than the oolitic limestones that underlie it and preceded it in time. And since the Inferior Oolite and Fullers Earth tell us that the sea had in their day crept far up the flanks of Mendip, it would seem probable that, by further subsidence of the land, the island completely sank beneath the deeper waters of the Oxford Clay sea. In any case, it is not probable that Mendip was long submerged; and when ere long (perhaps in the age of the Portland Oolites) upheaval again lifted the area above the waters, it must have formed a fertile lowland district, contrasting strongly with the mountainous parts of Cornwall, Devon, and Wales, which had not been thus submerged and smothered in Jurassic deposits.

I must now content myself with sketching very briefly, in a couple of paragraphs, the events which led up to the second submergence and the final re-emergence of the Mendips.

Probably the upheaval that re-elevated the Mendips above the waters of the oolitic sea converted nearly the whole of England north of London and the Vale of Wardour into a rich and fertile land surface, bordering an eastern sea which occupied the site of part of the North Sea, and formed a Speeton Bay over parts of Yorkshire and Lincolnshire. To the west there was, in Mr. Jukes-Brown's opinion, a lake in the area which is now the Irish Sea between the Isle of Man, Anglesea, and Lancashire. To the south lay an inland sea, soon to be converted into a Wealden Lake (Map 5), stretching across the Channel into Northern France, and receiving the waters of great rivers flowing eastwards through the English and Bristol Channels. On the flats by the rivers there were mares-tails in the marshes, ferns, cycads and coniferous trees on the drier ground. Crocodiles, turtles, and fish swarmed in the waters; leathern-winged pterodactyles flitted through the air; small marsupial animals dwelt upon the land; and over the marshy flats waddled the unwieldy Iguanodon.



Submergence again set in, and the sea which rolled over

Southern France found access to the Wealden Lake, converting it into a Vectian Gulf. The waters of the Speeton Bay were also creeping up over the land, and beating on the northern shore of a barrier extending from Belgium through London and south-central England. This barrier separated the northern waters of the Speeton Bay from the southern waters of the Vectian Gulf, and as submergence continued, this barrier grew narrower and narrower until the two were united by straits between Berkshire and Cambridgeshire. The straits widened and deepened as England underwent her last great period of subsidence beneath the waters of the Chalk Ocean, which teemed with the myriad life of minute marine animalcules, the tiny shells of which sinking to the bottom gave rise to a greyish calcareous ooze, which, now that it is uplifted to form our downs and southern shore cliffs, we term chalk. Dartmoor, parts of Wales, and the Cumbrian Lake Mountains, perhaps alone of all England appeared as islands in the midst of the blue waters of the warm clear sea. The Mendips were once more, and for the last time, completely submerged.*

When they again rose from the waters they were overmantled by a deposit of pure white chalk. But even as they emerged from the ocean the waves were busy rapidly removing the soft calcareous ooze, and when they had passed through this ordeal and were lifted up once more into the atmosphere, they were forthwith subjected to the action of rain, the weather and a thousand streamlets. Thus were all the more recent deposits stript from the Mendip axis, and the old scenic features, so long buried, were again disclosed, and received under the chisel of denudation their final sculpturing. Around them, mantling their flanks, still lie the remnants of the deposits of the geological middle age.

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* Near Frome the Wealden and Lower Cretaceous are absent. The Upper Greensand lies directly on an eroded surface of Oxford Clay.

From that time to the present the Mendips have remained above the waters. They have looked out on many more recent geological changes. They have seen, in Tertiary times, spreading over London, Hampshire, and the Isle of Wight, the warm waters of a tropical bay or estuary, in which a host of crocodiles lay basking, and turtles lazily disported themselves over London, untroubled by the nightmare visions of aldermanic feasts. They have seen England, then joined to the Continent, the home of huge mammals of strange and uncouth aspect. They have seen the coast of Suffolk bordered by a sea, growing colder and colder, and colonized by a constantly increasing number of arctic shells which migrated southwards before the advancing ice. They have seen the north of England and Scotland buried under glaciers and vast accumulations of land ice or partially submerged under an ice-laden sea. Their own flanks may have been torn, and their gorges and ravines scoured out and deepened by torrential rivers due to the melting of the snow with which they were themselves covered. They have seen early man forced to migrate before the advancing ice, contending against fierce beasts of prey, or hunting the milder equines and bovines, armed with rudely fashioned weapons of flint and stone.

And here the Mendips themselves are able to afford their item of evidence; for in caves hollowed out in the limestone rock there are found the bones and teeth of the animals which then ranged over the Mendips and the extensive valley flats which fringed them to the south and north. Into some of these caves, such as that at Banwell, the bones were washed from the surface by streams which disappeared down swallet-holes. Others were hyæna dens into which these creatures dragged their prey. From time to time primitive man ousted the hyænas from their dens and took summary possession thereof.

"We may picture to ourselves," says Mr. Boyd Dawkins, "a fertile plain occupying [a considerable area of] the Bristol Channel, and supporting herds of reindeer, horses, and bisons, many elephants and rhinoceroses, and now and then being traversed by a stray hippopotamus, which would afford abundant prey to the lions, bears, and hyænas inhabiting all the accessible caves, as well as to their great destroyer—man. . . . Hyænas were the normal occupants of the caves (*e.g.* Wookey Hole) and thither they brought their prey. We can picture these animals pursuing elephants and rhinoceroses along the slopes of the Mendips, till they scared them into the precipitous ravine (Cheddar Gorge), or watching until the strength of a disabled bear or lion ebbed away sufficiently to allow of its being overcome by their cowardly strength. Man appeared from time to time on the scene, a miserable savage, armed with bow and spear and unacquainted with metals. Sometimes he took possession of the den and drove out the hyænas. He kindled his fires at the entrance, to cook his food and to keep away the wild animals; then he went away, and the hyænas came back to their own abode."

The "miserable savage" of whom Mr. Boyd Dawkins here speaks, belonged to the old Palæolithic folk who ranged the Mendips when England was continental, and when Scotland and the North had not yet shaken off the chill ice-pall of the glacial epoch. Without the aid of the dog they lived as best they could by the chase, armed and appointed with rudely finished chipped implements fashioned from such flints as they found ready to their hand. But the Mendips, which had already witnessed so many changes, saw this race pass away, to be after many days replaced by Neolithic folk.

Within sight of the Mendips, at Stanton Drew, there stands to this day, formed of huge brecciated blocks, some of them carried thither from Mendip, a system of stone circles erected, as I believe, by these Neolithic folk. England had, in their day, become insular, and the glacial epoch was a thing of the past. Unlike the Palæoliths, who were acquainted with many animals unfamiliar to us, and some of them extinct, the Neolith knew only one extinct animal, the great Irish deer, noblest-antlered of his kind. In their hunting expeditions the dog was at their side. They ground and polished their implements, and mined for the flints of which they were fashioned. Above all they were herdsmen and farmers, who introduced many of our cerials and domestic cattle.

These were perhaps the pre-Aryan inhabitants of Britain. They were in course of time invaded by Aryan folk who brought with them the use of bronze, and all the varied culture of the axe; who buried their chieftains in the round barrows which still dot the Mendip uplands, and who, in later days, had, in turn, to give way before the Romans, the scars of whose mining operations still seam the sides of Mendip.

The sun was sinking and the air was growing chill, as I grasped again my geological hammer, and swiftly descended from the Mendip upland.

The Stones of Stanton Drew: their Source and Origin.

BY PROF. C. LLOYD MORGAN.

IN the "Proceedings of the Somersetshire Archæological and Natural History Society (1887)" I have given a full account of my investigations at Stanton Drew, and have published a plan of the circles (modified from that of Mr. Dymond, C.E., F.S.A.) with references to their lithological character. From the paper there published I here quote, by permission, a few paragraphs.

Concerning the megalithic remains at Stanton Drew much has been written. Local tradition has preserved for us an account of their origin sufficiently miraculous. Around them in later times there has been a delicate play of archæologic fancy.

In this paper it is not my purpose to criticize or to discuss at any length the final cause of their erection. The task I have set before myself is a more practical, and, I venture to hope, a more useful one. My object in the investigations, the imperfect results of which are here with some diffidence laid before the Somersetshire Archæological and Natural History Society, has been—(1) To ascertain the nature of the rocks of which the stones are composed; (2) To ascertain where such rocks may now be found *in situ;* and thus (3) To ascertain whence the ancient Neolithic folk (for by them I believe the stone circles to have been erected) brought these giant stones.

The Nature of the Stones.—In addition to the stones of the Great Circle and its Avenue, the North-east Circle and its Avenue, and the South-west Circle, there are three stones, known as the Cove, situated near the Church; there are two small stones in the Middle Ham or Lower Tyning, about one thousand yards west (and a little north) of the Great Circle; and there is one large stone (Hautville's or Hackwell's Quoit), about six hundred yards east-north-east of the Great Circle.

A cursory examination of the stones shows that they are not all composed of the same rock-material. The majority of them are, as has often been pointed out, of a very peculiar nature, being composed of a highly silicious breccia, full of angular fragments, of various sizes and shapes, embedded in a reddish silicious matrix, freely impregnated with iron. The rock is also full of hollows, some of which are lined with crystallized quartz, while others are completely filled up with this material. The embedded fragments have also a curious banded appearance; the banded layers running parallel with the contour of the fragments. The stones of this class exhibit considerable variety of structure and external appearance; some are composed throughout of a close red or brown cherty material, with but few embedded fragments, and scarcely any hollows. Others have many larger or smaller hollows, and have a rough and slaggy appearance, giving rise to the popular but erroneous idea that they are of volcanic origin. Collinson might well be excused for calling some of these rock masses "a composition of pebbles, grit and other concrete matter," and doubting that they were "ever hewn from the rock." I shall speak of the rock of which these stones are composed as Silicious Breccia.

Besides the stones which are composed of this Silicious Breccia, there are others, five in number, which are composed of a Dolomitic Breccia, in which comparatively small fragments of (Mountain) Limestone are embedded in a reddish matrix, containing iron and carbonate of lime. This has, so far as I know, never been differentiated from the Silicious Breccia by previous observers. It is, however, a distinct rock; and the fact that all three stones of the Cove are composed of it is, I think, noteworthy.

The two small stones in the Lower Tyning, as well as one, perhaps two, in the Great Circle, and one in the North-east Circle Avenue, are a yellowish Limestone. The presence of an Echinoid in one of the stones in the Lower Tyning marks this rock as belonging to the Oolite series of geologists.

Four stones are composed of Sandstone: the Quoit, two in the Great Circle, and one in the South-west Circle. I think it not unlikely that the Sandstones in the circles are of Palæozoic age, perhaps Old Red Sandstone. But that of the Quoit is of a different and closer character.

Thus, if we separate these Sandstones, there are five distinct kinds of rocks—Silicious Breccia, Dolomitic Breccia, Oolitic Limestone, coarser Sandstone, and the close fine-grained cherty Sandstone of the Quoit.

Whence were these severally brought?

THE SOURCES OF THE STANTON DREW STONES.—1. *The Silicious Breccia.*—Although the variable nature of this rock makes it impossible to say, for certain, from what exact spot this rock was brought, its peculiar and local character enables us to say, with tolerable certainty, that it was obtained either from the neighbourhood of Harptree under Mendip or from Leigh Down, on the eastern skirt of Broadfield Down, or perhaps from both these localities.

I feel very little doubt that all the stones of the North-east Circle (Circle of Eight) are from the Harptree neighbourhood. The stones which seem to me to be from Leigh Down, near Winford, are indicated in the paper I have referred to.

2. *Dolomitic Breccia.*—Unless we are to go yet further afield, this rock, too, was obtained either from the skirts of Broadfield Down or from the Mendip margin. As before mentioned, flat slabs, similar to those in the Cove, are found near Rudd and on Green Down. But I do not think we are restricted to these localities.

3. *The Limestone.*—For some time I was doubtful about the source of the stones composed of this rock. It is very difficult to determine from a weathered surface, and I have not felt justified in chipping any of the stones. From the occurrence of an Echinoid in one of the stones of the Lower Tyning, the weathered surface of which resembles that of the other limestone monoliths, I am now disposed to refer them to the Inferior Oolite of Dundry.

4. *The Coarser Sandstone.*—As to the exact locality whence these stones were obtained, I am not at present prepared to offer an opinion. I am inclined to regard them as Palæozoic; but even of this I would not speak too positively.

5. *The Fine-Grained Sandstone.*—Of the source, geological and local, of this rock I am doubtful.

It is possible that one or more of the Sandstone monoliths may be Sarsen—but whence?

Conclusion.

The following facts seem to come out definitely from the investigations here recorded.

(1) That the stones of the North-east Circle, containing the largest monoliths, are all of one kind (Silicious Breccia), and probably all from one source—the Harptree neighbourhood; (2) That the great Circle and South-west Circle are composed of smaller stones of diverse origin; (3) That the stones in the Cove are of one kind of rock (Dolomitic Breccia), which differs from that whereof the stones of the North-east Circle are composed, and of which there is only one stone in the Great Circle and one in the South-west Circle.

I think it may fairly be inferred from these facts, that the North-east Circle is of different date* from that of the other circles, and that the Cove is also of different date. Whether the North-east Circle of larger monoliths is older or later than the Great Circle, with its smaller diverse monoliths, and what is the relative date of the Cove, I do not pretend to say. It is a matter of mere speculation whether the smaller circle of large monoliths, or the larger circle of small monoliths, was the earlier. I imagine, however, that the circles were of gradual growth.

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* When I say of different date, I do not mean to imply, erected by a different race or tribe.

Rainfall at Clifton in 1887.

By GEORGE F. BURDER, M.D., F.R. Met. Soc.

TABLE OF RAINFALL.

	1887.	Average of 35 years.	Departure from Average.	Greatest fall in 24 Hours.		Number of days on which ·01 in. or more fell.
				Depth.	Date.	
	Inches.	Inches.	Inches.	Inches.		
January. .	2·271	3·320	—1·049	0·455	19th	15
February	0·696	2·293	—1·597	0·275	3rd	7
March . .	2·382	2·186	+0·196	0·676	15th	9
April. . .	1·956	2·079	—0·123	0·751	26th	11
May . . .	2·337	2·434	—0·097	0·644	31st	14
June . . .	1·001	2·555	—1·554	0·790	2nd	4
July . . .	1·125	2·911	—1·786	0·322	26th	11
August . .	2·938	3·451	—0·513	1·091	31st	8
September .	3·422	3·373	+0·049	0·688	1st	15
October . .	2·481	3·724	—1·243	1·031	29th	10
November .	2·772	3·017	—0·245	0·388	5th	18
December .	2·412	2·881	—0·469	0·420	15th	18
Year . . .	25·793	34·224	—8·431	1·091	Aug. 31st	140

REMARKS.—The year 1887 was the driest year since 1870, the amount of rain having fallen short of the average of 35 years by nearly 8½ inches, or about a fourth part of the whole. Before 1870 dry years were of more frequent occurrence; and out of the series of 35 years during which observations have been taken, no less than five have had a rainfall smaller than that of last year. The least annual fall was in 1864, namely, 22·746 inches. There has, however, been no parallel in 35 years to the deficiency of rain which was experienced in the interval from the 3rd of June to the 29th of August in the past year. The total fall in that period of nearly three months was 2·386 inches. The two nearest approaches in former years to this prolonged drought occurred in 1864 and 1869. In 1864 the quantity collected in the same interval was 3·836 inches; in 1869 it was 3·322 inches.

Specially dry periods of shorter duration were noted in 1887 as follows:—From February 3rd to March 13th, 38 days with less than four-tenths of an inch of rain; March 26th to April 21st, 26 days with less than three-tenths; June 2nd to July 12th, 40 days with less than half an inch; July 26th to August 15th, 20 days practically rainless. The longest absolute drought was from June 8th to July 4th —a period of 26 days unbroken by a shower.

The driest month in 1887 was February, with barely seven-tenths of an inch of rain. The wettest was September, with nearly 3½ inches; but this quantity was only slightly in excess of the average for that month. March and September were the only months in the year which showed no deficiency.

The great snowstorm of March 15th, 1887, claims record here, as being the most remarkable for level depth of snow that has occurred in this part of the country for probably half a century. The average depth was 15 inches, yielding when melted $1\frac{1}{4}$ inch of water, which was more than half of the entire downfall of that month. The snow was of very light texture, and not much drifted; hence the damage and inconvenience sustained were less than on some other occasions when the level depth has not been nearly so great.

Meteorological Observations,

AS REGARDS

Temperature, taken at Clifton, 1887.

By D. RINTOUL, B.A., Cantab.

THE following table contains some results of observations carried on at Clifton College during the year 1887. The conditions of the Royal Meteorological Society are complied with in all the observations. It will be seen that the year was distinguished by an abnormally large number of frosts, as indicated by a minimum thermometer placed about 4 inches above ground covered with grass. The minimum temperature recorded (11·7° F.) is also lower than for any year since 1881.

Owing to an accident, the maximum shade temperatures were recorded for only part of January, so that the results for that month are not complete. In comparing the various months of 1887 with those of the previous six years, it will be seen that the temperature in January, June, and July was higher than the average, while March, April, May, September, October, November, and December were colder, May especially being distinguished by low mean temperature.

1887 TEMPERATURES.

MONTH.	Maximum in Shade.		Minimum in Shade.		Mean in Shade.	Minimum on Ground, Lowest recorded.
	Highest recorded.	Mean.	Lowest recorded.	Mean.		
January .	53·5	50·1	20·4	33·0	41·5	15·1
February .	56·9	46·7	24·4	35·3	41·0	16·7
March . .	59·2	45·4	23·8 (?)	34·4	39·9	11·7
April . .	63·3	49·0	29·6	35·3	42·2	18·9
May . . .	70·1	57·5	36·2	41·5	45·5	26·0
June . .	82·5	78·6	43·3	46·2	62·4	35·0
July . . .	82·8	73·5	49·0	54·3	63·9	35·0
August .	80·6	68·1	45·2	52·6	60·4	40·8
September	65·9	59·3	33·2	48·1	53·7	31·7
October .	60·3	52·1	28·0	40·1	46·1	22·8
November	51·3	46·1	25·4	36·2	41·2	20·1
December.	52·3	45·5	23·4	34·3	39·9	18·3
Year 1887.	82·8	56·0	20·4	40·9	48·4	11·7

Year 1886.	83·5	54·90	21·7	43·17	49·03	15·3
Year 1885.	87·8	53·98	22·1	42·53	48·09	20·1
Year 1884.	87·5	57·44	22·6	44·07	50·66	23·7
Year 1883.	82·5	54·54	20·9	42·88	48·71	19·3
Year 1882.	78·5	55·46	21·9	43·62	49·54	20·6
Year 1881.	86·9	55·44	12·3	42·92	49·18	5·8

MONTH.	Number of Days on which the Minimum Ground Temperature was below 32°F.	Number of Days on which the Minimum Air Temperature was below 32°F.	Number of Days on which the Maximum Air Temperature was below 32°F.	Number of Days on which the Mean Air Temperature was below 32°F.
January . .	19	14	0	0
February . .	18	11	0	1
March . . .	26	14	1	4
April . . .	27	3	0	0
May . . .	10	0	0	0
June . . .	0	0	0	0
July . . .	0	0	0	0
August . .	0	0	0	0
September .	2	0	0	0
October . .	10	3	0	0
November .	14	10	0	2
December .	22	8	1	4
Year 1887 .	148	63	2	11
Year 1886 .	102	64	1	22
Year 1885 .	68	40	1	6
Year 1884 .	51	19	0	1
Year 1883 .	79	40	0	6
Year 1882 .	63	26	2	7
Year 1881 .	94	60	11	24

MEAN SHADE TEMPERATURES OF THE MONTHS.

MONTH.	1881.	1882.	1883.	1884.	1885.	1886.	1887.	Mean of Seven Years.
January . .	31·8	40·7	42·3	44·2	38·7	35·5	41·5	39·24
February .	39·3	42·6	43·3	42·3	44·0	35·8	41·0	41·19
March . .	43·0	45·7	37·0	44·9	41·7	40·1	39·9	41·76
April . . .	47·3	48·8	47·5	45·1	46·7	46·4	42·2	43·43
May . . .	54·4	53·7	49·9	53·3	47·0	52·2	45·5	50·94
June . . .	56·9	56·0	57·2	59·8	58·4	58·8	62·4	58·5
July . . .	65·9	59·6	57·2	60·7	62·6	62·7	63·9	61·8
August . .	58·8	60·2	60·6	64·5	57·5	59·1	60·4	60·16
September .	56·6	53·9	55·3	59·4	54·5	58·2	53·7	55·95
October . .	46·5	50·2	49·6	48·9	45·6	52·9	46·1	48·54
November .	48·3	43·7	42·8	41·6	43·3	47·1	41·2	44·00
December .	40·8	39·8	41·4	43·1	38·8	38·6	39·9	40·34
The Year .	49·2	49·5	48·7	50·7	48·1	49·0	48·4	49·08

Elimination and Selection.

BY PROF. C. LLOYD MORGAN.

Read April 5th, 1888.

THOSE who have read the recently-published "Life of Charles Darwin" may remember a footnote in which Mr. A. R. Wallace criticizes the phrase "Natural Selection." "The term 'Survival of the Fittest,'" he says, "is the plain expresssion of the fact; 'Natural Selection' is a metaphorical expression of it, and to a certain degree indirect and incorrect, since Nature does not so much select special varieties as exterminate the most unfavourable ones."* Mr. Darwin, while admitting with his wonted candour the force of this criticism, urges in support of the use of his own phrase, first, that it can be employed as a substantive governing a verb; secondly, that it serves to connect artificial and natural selection; and thirdly,† that its meaning is *not* obvious, and that this leads men to think the matter out for themselves.

I propose here briefly to consider Mr. Wallace's criticism; to suggest provisionally the use of the phrase, "Natural Elimination," which *can* be employed as a substantive "governing a verb"; and to indicate the advantages which would attend the use of such a term, not the least of which is, that it serves to distinguish between artificial selection and "natural selection."

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* "Life," vol. iii., p. 46.

† Vol. ii., p. 278.

Mr. Herbert Spencer's term, "Survival of the Fittest," says Mr. Wallace, is the plain expression of the fact; "Natural Selection" is a metaphorical expression of it. Yes; but in the first place, Mr. Spencer's phrase gives no inkling of the process by which such survival is brought about; and, in the second place, it is questionable whether any phrase, which does so indicate the process, can escape the charge of being in some degree metaphorical. The sting of Mr. Wallace's criticism, therefore, would appear to lie (appropriately) in the tail, where he points out that Nature does not so much select special varieties as exterminate the most unfavourable ones. This seems to me a valid criticism; one which Mr. Darwin does not sufficiently meet; and one which still holds good. I would, however, venture to suggest that the word "eliminate," though somewhat metaphorical, is more satisfactory than Wallace's word, "exterminate"; and I further venture to suggest that the use of the phrase, *Natural Elimination*, would emphasize the fact that, whereas in artificial selection it is almost invariably the fittest which are chosen out for survival, it is not so under Nature; the "survival of the fittest" under Nature being in the main the net result of a slow and gradual process of the elimination of the unfit. The well-adapted are not selected; but the ill-adapted are rejected; or rather, the failures are just inevitably eliminated.

I do not mean for one moment to hint that Mr. Darwin failed to recognise this fact. But I do think he failed to give it adequate expression. I do think that if he had employed the term "Selection" for the choosing out the more fit, and "Elimination" for weeding out the less fit, his meaning in many cases would have been made more clear. "The principle of selection," he says, "may be conveniently divided into three kinds: *Methodical Selection* is that which guides a man who systematically endeavours to modify a breed according to some pre-determined standard. *Unconscious Selection* is that which follows from men naturally preserving the most valued, and destroying the less valued individuals, without any thought of altering the breed. Lastly, we have *Natural Selection*, which implies that the individuals which are best fitted for the complex and in the course of ages changing conditions to which they are exposed, generally survive and procreate their kind."* Here the transition from selection to elimination is effected under the head of unconscious selection, where the breeder is not intentionally modifying the strain, but is merely desirous of keeping up the standard. And this he effects in one or both of two ways: either by selecting his best cattle, or dogs, or other domestic animals to breed from, or by weeding out the unsatisfactory individuals. The end in view is the same, but the processes employed are sufficiently distinct; selection being applied to one end of the scale, elimination at the other.

Now in "natural selection" (so-called), the standard is maintained mainly (but not entirely) by weeding out the failures; by elimination of the unfit. "Natural Rejection" would therefore have been a more appropriate phrase; but "Natural Elimination" seems to me more satisfactory and less metaphorical.

It is just possible that some one may say: If nothing more takes place than the elimination of the unfit, where is the possibility of advance? You may keep up the standard, but progress is surely impossible. Such an objection would, however, imply a forgetfulness of the facts of variation. Variation is constantly taking place; and the variations may be favourable, or unfavourable, or neutral. Under selection, the favourable variations will be chosen out; the unfavourable and the neutral may go. Under elimination, the unfavourable disappear; the favourable *and the neutral* remain. By how much the favourable variations are in excess, by so much will the race tend to advance. I see no reason why neutral variations should be eliminated, except in so far as,—in the keen struggle for existence,—they become relatively unfavourable.*

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* "Animals and Plants," 1st ed., vol. ii., p. 193.

Too much stress is, I think, laid upon utility. Mr. Wallace himself contends "that none of the definite facts of organic nature, no special organ, no characteristic form or marking, no peculiarity of instinct or of habit, no relations between species or between groups of species, can exist, but which must now be or once have been *useful* to the individuals or the races which possess them." † And Mr. Romanes, in his valuable and suggestive paper on Physiological Selection (physiological *isolation* would better express its scope), brings forward the inutility of specific characters as one of the three "cardinal difficulties in the way of natural selection, considered as a theory of the origin of species." "The features," he says, "which serve to distinguish allied species, are frequently, if not usually, of a kind with which natural selection can have had nothing whatever to do; for distinctions of specific value frequently have reference to structures which are without any utilitarian significance." ‡

But why should neutral variations,—variations, that is to say, which are neither useful nor harmful,—be eliminated under Nature? If they escape, through isolation or otherwise, that swamping by intercrossing by which *all* variations are liable to be submerged, why should they be weeded out? I am inclined to think that the use of the term "Natural Selection," implying some natural tendency for the fittest individuals and the most useful structures to be chosen out for preservation, has led to too much stress being laid on the necessary utility of structures and specific features. And I venture to think that the use of some such term as "Natural Elimination," implying the natural tendency of the unfavourable and deleterious variations to be weeded out, would have saved us from some perplexity in this matter. Undoubtedly, in the long run, useful variations will tend more and more to preponderate, since the longer and keener the struggle the greater and more inevitable the tendency of neutral variations to become relatively unfavourable. And this is exactly what we do find. For, as Mr. Romanes remarks, in continuation of the passage quoted above, "It is not until we advance to the more important distinctions between genera, families, and orders that we begin to find, on any large or general scale, unmistakable evidence of utilitarian meaning."

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* Cf. "Origin of Species," 6th ed., p. 63.

† "Natural Selection," p. 47.

‡ *Journ. Lin. Soc., Zool.*, vol. xix., p. 338.

Not only does the use of the phrase "Natural Elimination" save us from misconceptions of this nature; it also serves to connect the natural process with that struggle for existence out of which it arises. The struggle for existence is the reaction of the organic world called forth by the action of natural elimination. Organisms are tending to increase in geometrical ratio. There is not room for the myriads born. The tendency to increase is therefore held in check by elimination involving the struggle for existence

"This term," says Mr. Darwin, "I use in a large and metaphorical sense," which he then proceeds to explain. It would seem, in the suggested phraseology, to be the result of a three-fold process of elimination. First, elimination by the direct action of surrounding conditions; secondly, elimination by enemies; and thirdly, elimination by competition.*

As an example of the first kind of elimination, Darwin tells us that in the winter of 1854-5, four-fifths of the birds in his grounds perished from the severity of the weather. My colleague, Mr. Munro Smith, informs me that, in cultivating microbes, certain forms, such as *Bacillus violaceus* and *Micrococcus prodigiosus*, remain in the field during cold weather when other less hardy microbes have perished. At the edge of a coral reef, minute, active, free-swimming coral embryos are set free in immense numbers. Presently they settle down for life. Some settle on a muddy bottom, others on a cold bottom, others at too great a depth. All these are eliminated. Only the few who chance to take up a favourable position are preserved. The parable of the Sower gives us another case in point. Examples could be multiplied indefinitely. I imagine that during the oncoming of the glacial epoch there was much pitiless elimination of this order. Among civilized human folk this form of elimination is only seen in military campaigns, in Arctic expeditions, and in arduous travels. But in early times and among savages it must be a more important factor.

Elimination by enemies scarcely needs exemplification. Battle within battle must, throughout nature, as Mr. Darwin says, be continually recurring with varying success. The stronger devour the weaker, and wage war with each other over the prey. Under this head may be taken the phenomena of parasitism. Neither cattle, nor horses, nor dogs have ever run wild in Paraguay, owing to the insidious attacks of a certain fly, which lays her eggs in the bodies of the newly born. There is scarcely a form of life so harmless or so retiring as not to be liable to the attacks of enemies from without or from within. Among human folk, moreover, elimination by enemies is not wholly unknown; and in this connection it is a sad reflection, as Sir W. R. Grove has well said, that man is almost the only animal that fights, not for food, or means of life, or of perpetuating its race, but from motives of merest vanity, ambition, or passion.

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* Cf. "Origin of Species," pp. 50 and 53.

Elimination by competition is by far the most important. As Mr. Darwin so well points out, the competition is keenest between members of the same group and among individuals of the same species, or between different groups or different species which have, so to speak, similar aims in life. Alternations of hard times and good times are here effective, and may convert competition into war. During the Exhibition at South Kensington there were good times for rats. But when the show was over, there followed times that were cruelly hard. The keenest competition for the scanty food arose; and the poor creatures were forced to prey upon each other. "Their cravings for food," we read in *Nature*, "culminated in a fierce onslaught upon one another, which was evidenced by the piteous cries of those being devoured. Their method of seizing their victims is to suddenly make a raid upon one weaker or smaller than themselves, and after overpowering it by numbers, to tear it in pieces."

During the upheavals and depressions and the marked climatic changes of geological times, this alternation must have occurred again and again. Not only would there be an actual expansion and contraction of the life-area, as when a continent was rising or sinking, but there would be a virtual expansion and contraction as the power of supporting life in the area was, by changes of climate or other causes, increased or diminished. During good times, varieties which would otherwise be unable to hold their own might arise, and have time to establish themselves. During bad times, all who were then found unfit would be eliminated.

That elimination by competition obtains among human folk, needs, alas! no illustration. Here, too, there is an alternation of good times and hard times, with effects sufficiently marked. The introduction of ostrich-farming in South Africa affords a case in point. This produced good times for the farmers. Whereupon there resulted variation in two directions. Some devoted increased profits to improvements on their farms, to irrigation works which could not before be afforded, and so forth. For others, increased income meant increased expenditure, and an easier, if not more luxurious, mode of life. Then came hard times. Others, in Africa and elsewhere, learnt the secret of ostrich-farming. Competition brought down prices, and elimination set in—of which variety need hardly be stated.

Such, then, are the modes of elimination. Observe that it is a differentiating process. As Darwin says: "It may be well here to remark, that with all beings there must be much fortuitous destruction, which can have little or no influence on the course of natural selection."* The ant-bear swallowing a tongue-load of ants; balænoptera engulfing whole shoals of herrings; the Greenland whale swallowing thousands of fry; the bear or the badger destroying whole nests of bees—these are examples of wholesale destruction, not of the elimination of the unfit.

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* "Origin of Species," p. 68.

Let us now turn to cases of selection, properly so called, where Nature is in some way working at the other end of the scale; where her method is not the elimination of the unfit, but the selection of the fit. Such a case may be found on Darwin's principles in brightly-coloured flowers and fruits. "Flowers," he says, "rank amongst the most beautiful productions of nature; but they have been rendered conspicuous in contrast with the green leaves, and, in consequence, at the same time beautiful, so that they may be easily observed by insects. I have come to this conclusion from finding it an invariable rule, that when a flower is fertilized by the wind, it never has a gaily coloured corolla. Several plants habitually produce two kinds of flowers; one kind open and coloured, so as to attract insects; the other closed, not coloured, destitute of nectar, and never visited by insects. Hence we may conclude that, if insects had not been developed on the face of the earth, our plants would not have been decked with beautiful flowers, but would have produced only such poor flowers as we see on our fir, oak, nut, and ash trees, on grasses, spinach, docks, and nettles, which are all fertilized through the agency of the wind. A similar line of argument holds good with fruits; that a ripe strawberry or cherry is as pleasing to the eye as to the palate,—that the gaily coloured fruit of the spindle-wood tree, and the scarlet berries of the holly, are beautiful objects,—will be admitted by every one. But this beauty serves merely as a guide to birds and beasts, in order that the fruit may be devoured and manured seeds disseminated: I infer that this is the case from having as yet found no exception to the rule, that seeds are always thus disseminated when embedded within a fruit of any kind (that is, within a fleshy or pulpy envelope), if it be coloured of any brilliant tint, or rendered conspicuous by being white or black." *

Here we have a case of the converse of elimination, a case of genuine selection under nature. But even here the process of elimination also comes into play, for the visitations of flowers by insects involves cross-fertilization. The flowers of two distinct individuals of the same species of plants in this manner fertilize each other; and the act of crossing, as Mr. Darwin so exquisitely proved, gives rise to vigorous seedlings, which consequently would have the best chance of flourishing and surviving—would best resist elimination by competition. So that we here have the double process at work; the fairest flowers being selected by insects, and those plants which failed to produce such flowers being eliminated as the relatively unfit.

If we turn to the phenomena of what Mr. Darwin termed sexual selection, we find both selection and elimination brought into play. By the law of battle, the weaker and less courageous males are eliminated so far as the continuation of their kind is concerned. By the individual choice of the females (I may not here say the fairer sex), the finer, bolder, handsomer, and more tuneful wooers are selected.

Let us again hear the voice of Mr. Darwin himself. "Most male birds," he says, "are highly pugnacious during the breeding season, and some possess weapons especially adapted for fighting with their rivals. But the most pugnacious and the best-armed males rarely or never depend for success solely on their power to drive away or kill their rivals, but have special means for charming the female. With some it is the power of song, or of emitting strange

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* "Origin of Species," p. 161.

cries, or of producing instrumental music; and the males in consequence differ from the females in their vocal organs or in the structure of certain feathers. From the curiously diversified means for producing various sounds, we gain a high idea of the importance of this means of courtship. Many birds endeavour to charm the females by love-dances or antics, performed on the ground or in the air, and sometimes at prepared places. But ornaments of many kinds, the most brilliant tints, combs and wattles, beautiful plumes, elongated feathers, top-knots, and so forth, are by far the commonest means. In some cases, mere novelty appears to have acted as a charm. The ornaments of the males must be highly important to them, for they have been acquired in not a few cases at the cost of increased danger from enemies, and even at some loss of power in fighting with their rivals.* . . . What, then, are we to conclude from these facts and considerations? Does the male parade his charms with so much pomp and rivalry for no purpose? Are we not justified in believing that the female exerts a choice, and that she receives the addresses of the male who pleases her most?" †

Here again, then, we have the combined action of elimination and selection. And now we may note that selection involves intelligence; or, since it may be objected that selection is in some cases instinctive, let us rather say, involves the mind-element, or the element of consciousness. Hence it is that when we come to consider the evolution of human folk, the principle of elimination is so profoundly modified by the principle of selection. Not only are the weaker eliminated by the inexorable pressure of competition, but we select the more fortunate individuals and heap upon them our favours. This enables us also to soften the rigour of the blinder law; to let the full stress of competitive elimination fall upon the worthless, the idle, the profligate, and the vicious; but to lighten its incidence on the deserving but unfortunate.

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* "Descent of Man," summary of chap. xvi., pt. ii.

† *Ibid.* chap. xiv.

Too little importance has, perhaps, been attached of late years to the mental element in evolution. In Lamarckism it took a foremost place. "Every considerable alteration in the local circumstances in which each race of animals exists," wrote Lamarck, as summarized by Lyell, "causes a change in their wants; and these new wants excite them to new actions and habits. These actions require the more frequent employment of some parts before but slightly exercised, and their greater development follows as a consequence of this more frequent use. Other organs no longer in use are impoverished and diminished in size, nay, are sometimes entirely annihilated, while in their place new parts are insensibly produced for the discharge of new functions." * In the reaction against Lamarckism, the mental element fell into the background. But those naturalists who have kept abreast of philosophy are more and more coming round to the view that mind and body are indissolubly connected—that the mind does not act *ab extra*, but is an integral and essential part of the organic whole. In two ways is the mind-element operative; in enabling the intelligent organism to avoid elimination, and in furthering the process of selection.

I do not mean to imply that the mind-element can originate anything, except in reaction to surrounding conditions, inorganic and organic. We are still quite in the dark about origins. Elimination originates nothing; it merely crowds out failures. Selection originates nothing;

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* Lyell, "Principles," 11th ed., vol. ii., p. 253.

the favourable varieties must be there ere they can be chosen out for survival. When Darwin speaks of the eye as *formed by* natural selection, he uses, somewhat unguardedly, the language of metaphor. We are thrown back upon variation, bodily and mental, as the origin. But how originates this variation? In response to surrounding conditions. True; but how?

Starting, however, with variations, somehow conditioned and in some way caused, it has been my object to show that they are, under Nature, subjected to a double process—a process of elimination—weeding out the unfit, and a process of selection—choosing out the more fit. Of these, elimination is the more universal, selection only coming into play when intelligence has definitely appeared on the scene of life. Of the three kinds of variations—favourable, neutral, and unfavourable, elimination only gets rid of the unfavourable, leaving both the favourable and the neutral in possession of the field, except in those cases where severe and long-continued competition has rendered even the neutral variations relatively unfavourable. Selection, on the other hand, picks out only the favourable variations; so that under selection alone, the occurrence of useless structures or features would be anomalous. Both principles have been operative under nature; and both are included under Mr. Darwin's terms, "Natural Selection" and "Sexual Selection."

In conclusion, let me say that I am not so foolish or so vain as to suppose that what I have here written and elsewhere taught is likely to effect a revolution in biological nomenclature. Whether the occasional use of the term "Natural Elimination," alongside of and in subservience to Natural Selection, would be of service to those who are students and teachers of Evolution doctrines, I must leave others to judge.

The Structure, Decay, and Preservation of the Teeth.

By R. SHINGLETON SMITH, M.D., F.R.C.P., B.Sc.

[*Abstract.*]

A TOOTH was shown to be a modification of the horny epithelium of the skin intimately associated with a bony development from within: mammalian teeth being not simply horny, nor bony alone, but made up of several structures variously arranged in different animals. The characters of a typical simple tooth were described, and its transformations traced from its earliest period—the conical projection in the mucous membrane of the gum, which, being transformed into a layer of calcified bony dentine and covered with the epithelial enamel, forms the adult tooth. The process of development of the milk and the permanent teeth was given in outline, and the dates at which in infantile life the germs of the permanent teeth are being formed were indicated.

It was pointed out that the function of the teeth is mainly a mechanical one, that of mastication, in association with the chemical action of the salivary glands; but that nevertheless the structure of dentine gives indications of vital activity, nutritive changes being found therein, where-

as in enamel we have a highly calcified inorganic substance with no indication of physiological activity.

The principal evidence of vitality shown by dentine is its great sensibility when diseased; the presence of protoplasm in the tubular structure, and the fact that nerve filaments are found in it, derived both from the pulp and the periosteum, give reasons for this.

The vital characteristics of the dentine are most marked in intra-uterine life, and depend therefore on the health of the parent. Defects arising from this cause are added to by improper feeding in early life; but injury done at this period remains invisible, and only comes into prominence years afterwards, when the tooth appears above the gum. The various congenital defects of structure due to imperfect formation of the dental tissues were shown to be the principal predisposing causes on which decay of the teeth depends.

The conditions in the mouth which favour chemical activity, and the physiological activity of the tissues and ferment germs found there, were then reviewed. In health the tendency to disintegration is resisted, but morbid conditions favour the action of the disintegrating forces. The contrast, between the normal transformation of the teeth in old age, leading to their ultimate death and falling out with absorption of their supporting bony framework, with the premature decay and disintegration from caries in earlier life, was pointed out, the one a physiological but the other a pathological process.

Caries, or decay of the teeth, was described as a gradually progressive disintegration of the enamel and dentine, beginning always at the surface, generally in uneven rough places where food might lodge; the macroscopic and microscopic appearances were pointed out, more particularly the widening of the tubules of the dentine and the constant presence there of micrococci and bacteria. The microbes commonly found in the mouth, introduced soon after birth, inhaled and taken in with food and drink ever afterwards, and always teeming along the whole alimentary canal, had been shown to be intimately associated with dental decay: twenty-two different kinds had been described by Miller, of Berlin, as existing in the mouth, and sixteen of those were acid-producing, therefore aiding the disintegrating action of the acid fermentation of the saliva. The two fundamental conditions associated with caries:

a. Decalcification by acids,

b. Growth of microbes,

were then discussed. It was pointed out that the saliva is easily rendered acid by food decomposition; and the food, acting as a sponge, absorbs and retains the acid in the cracks and flaws on the surface of the enamel. This tissue, having little power of resistance to chemical influences, becomes destroyed by a process of mechanical and chemical erosion; this is the first step in dental caries, which must of necessity commence at the surface; but the tubular structure of the dentine will render it an easy prey to germ disintegration when once a way has been found for germ penetration through the covering of impermeable enamel. It was mentioned that caries can be produced in hippopotamus ivory under the ordinary conditions found in the mouth; but that acids alone will not produce it if germs be excluded.

As regards the great and increasing prevalence of dental caries, it was stated that the incidents and accidents of a lifetime are more than sufficient to account for tooth destruction before the advent of senile decay; the wonder is that teeth last as well as they do. Even if no congenital defects are present, the effect of all acute diseases, the accidents and diseases which influence the acidity of the buccal mucus,—dyspepsia and vomiting, for instance,—and defective nutrition of all kinds (*e.g.* anæmia, phthisis, diabetes) must be to favour the disintegrating action of the chemical and parasitic forces; where, however, the start in life has been bad, perfection in structure is impossible, and these influences will be still more harmful. The effect of the mechanical action of accumulation of tartar, the action of heated fluids and iced water, and inattention to dental cleanliness, are obviously potent for harm. The measures necessary to insure preservation of the teeth were classified as preventive and curative. Poorness of dental organization could only be remedied by pre-natal advice, and treatment calculated to insure a vigorous maternal system. The creation of people in accordance with the known axioms of physiology,—in fact scientific human breeding,—is as yet a dream of the future. Improved education will remedy much of the bad feeding and bad hygienic surroundings which deteriorate the health of early infancy, and lay the foundations of future caries. Local treatment may be summed up as absolute cleanliness; abolish fermentation from the mouth by chemical, mechanical, and antiseptic agencies. Antiseptics and antacids, with the aid of the tooth-brush, will do much to prevent what the dentist aims at stopping by processes which exclude air, fluids, food, and fungous growth.

Remarks about Seals,

AND THEIR SO-CALLED "BALLAST-BAG."

BY A. J. HARRISON, M.B. LON.

Read December 1st, 1887.

Seals are true *Mammalia.* Order, *Carnivora;* sub-order, *Pinnipedia*, (*a*) *Phocæ*, (*b*) *Otariæ.*

THE *Phocæ* are the seals most usually found in the northern hemisphere, the species *Vitulina* being the one found around our coasts. They are valuable for their skins, which are tanned into leather, and also, more or less, for the oil which their blubber or fat yields. The *Otariæ* belong to the southern hemisphere. They are much larger than the *Phocæ*, and have received the names of sea-lions, sea-bears, and sea-elephants, according to their supposed similarity to these animals respectively. The *Otariæ* are noted for their beautiful fur, which with their skin, tanned and prepared, is used to make ladies' mantles, muffs, etc., of; which, as most of us know, although very beautiful, are very costly.

Then I would like to remind you that these seals—and I am now using the term in a very general and inclusive sense—are animal feeders, true carnivora, only their chief food is not flesh but fish. Some of them however are not averse to capturing and devouring sea-birds, when they have the chance, which they seize very adroitly as the birds are swimming about. Certainly seals could never capture them on the land.

Further, if you examine the *dentition* of the carnivora, you will notice that they have a certain fixed arrangement of teeth: they have *incisors* in front of the jaw for grasping and holding their food; *canines*, usually four in number; and also *molars*, for cutting and triturating purposes. These molars vary in accordance with the varying work they have to perform, being sharper and more trenchant where mastication is slightly done. Seals have very trenchant molar teeth, a circumstance which I regard as of great importance.

I have alluded to a few of the leading characteristics of seals, but it is beyond the scope of my paper to go into any lengthened description of these creatures; but I may refer to the fact that they are intelligent and easily tamed, and then become very affectionate and demonstrative to their keepers. They can be induced to utter sounds, which are said to be very intelligible to those accustomed to them; and most, if not all, of the so named "talking fish" belong to this sub-order of the animal kingdom, and are not fish at all.

We have had from time to time many specimens of seals in our Zoological Gardens, and it has been a great source of pleasure to me to watch the habits and vagaries of these and our other animals; but I do not remember seeing more than one *species* of seal in the gardens, *viz.* the *Phoca vitulina* of the British coasts and of the Atlantic. Several specimens have been presented to us which have been taken off the shores of Newfoundland and Nova Scotia.

The specimen which is in the Gardens at the present time, and which is very lively and amusing, was taken in-the Wye, near Chepstow, about eighteen months ago. Seals are very fond of salmon—a good taste not limited to seals; and they frequently pursue this desirable prey up the estuaries and rivers for many miles. Fishermen know this predatory habit of seals but too well, for in pursuit of the salmon they often do great damage to the nets.

However the one now in captivity in the Gardens was, I suppose, too venturesome, and got trapped in the net, and was brought over to Clifton and purchased by the Society; and then became a companion to another animal, which had most kindly been presented by C. T. Bennett, Esq., the Newfoundland merchant of this city.

The two had not been companions very long when the latter one died. An examination of the body revealed the fact that its stomach was full of stones—the gravel chiefly of the Gardens—nuts uncracked, and pieces of hair and stick. The intestines were small and contracted, as though but little food had passed along them recently; and in some brief notes which I made at the time I entered the cause of death as being due (secondarily at least) to starvation or inanition. This I now believe to have been an incorrect statement, as the sequel, I think, will show; but anyhow I knew then no other cause of death, nor do I now.

During this last summer I visited a relative, the Rev. F. W. Bindley, Vicar of Gosforth, near Newcastle-on-Tyne, and who had lived for many years at the Cape of Good Hope. As a boy he took a deep interest in natural history, and inherited a fondness and aptitude for noticing the habits of birds and animals.

At the Cape there are or were very extensive seal fisheries, the Cape seal being the *Otaria pusilla*—one of the so-called sea-lions. These are a large species, and some of them display a magnificent growth of hair, which forms a regular mane, and gives them a very striking resemblance to a lion. At the Cape fisheries it was not an unusual thing for the fishermen to find, on opening these *Otariæ*, a bag containing a quantity of stones. Here is a good specimen, which I will hand round shortly. Now for what are these stones? What purpose do they subserve in the animal economy?

The tradition among the fishermen is, that these stones, or pebbles, are taken in by the animal to make "ballast"; and they assert moreover that this bag is not the stomach, but a separate receptacle, which they call the "ballast-bag." They say that when the seals get very fat they cannot sink easily in deep water, because specifically they become lighter, and hence to counteract this difficulty they swallow stones to increase their specific gravity. Now I am sure you will agree with me that this theory is a very pretty one—it is almost romantic; and if we can only back it up with facts or corroborative testimony, why I, for one, should have great pleasure in accepting it, swallowing it as easily as the seals seem to do these pebbles here.

Well, I was in conversation with Mr. Bindley one evening, and our talk was a good deal about birds and animals; for we had spent a most delightful time at the Newcastle Museum, where we had seen some of the most beautifully stuffed and arranged birds it has ever been my good fortune to see; especially I refer to those the work of Mr. Handcock, who is acknowledged to be the prince of bird-stuffers.

Suddenly my cousin said, "Have you ever seen the 'ballast-bag' of a seal?" I exclaimed, "No; what *do* you mean?" He said, "I will show you." And he then brought out this specimen which I have before me, told me its history, and of course the fishermen's pretty legend.

I was very much astonished, for I had never heard of such a thing before; but I naturally quickly associated my own little zoological experience with the Cape tradition, and I think you will agree with me that one seems to corroborate and confirm the other.

It appears that when the *Challenger* expedition, under the care of the late Sir Wyville Thomson, arrived at Cape Town in the autumn of 1873, Sir Wyville's attention was drawn to the tale about the "ballast-bag"; when he stated that he had examined many seals, but had never come across any such condition of things, and evidently was disposed to make light of the matter.

Some of the Cape scientists and others were rather annoyed at this scant treatment of their ideas; and after the departure of the *Challenger* a local commission was appointed to investigate the matter. This committee consisted of Mr. Bensuson, of the firm of Bensuson & Co., who had then the Cape fisheries; Mr. Ansdell, a merchant, of the firm of Leasight & Co.; Dr. McWalters, surgeon of the 86th Regiment; and Mr. Bindley. These gentlemen were all interested in the subject, and felt very indignant at Sir Wyville Thomson's way of treating it. Accordingly Mr. Bensuson and Dr. McWalters accompanied the next expedition of seal fishers. They opened a number of seals, the *Otaria pusilla*, and out of the number found several bags. When an opportunity occurred some specimens were sent to England, by a Cape medical student, Mr. Vanderbyl, who had instructions to place them in Sir Wyville Thomson's hands. This, I believe, was done; but unfortunately the speedy death of Sir Wyville, and then shortly afterwards of the medical student, closed the affair.

Now I have the pleasure of showing you a "ballast-bag," which was got during this same expedition—the preparation being now about twelve years old—and which was given me by Mr. Bindley this last summer. You will see at once, from the shape of it, that it is the true stomach of a seal, most probably the *Otaria pusilla*, but I have no positive information on this point. Thus you see here is the organ with the swallow-tube, or œsophagus, near the larger end; and here the much narrower part, ending in the pylorus, and being continued on into the small intestines.

In this dried condition the stomach and its contents weighed 2 lbs. 2 ozs.; the whole length is 14 inches, and the circumference around the largest part is 9½ inches. With the viscus in this dried condition you will perceive there is very little room for any food to be contained; but no doubt, during life, the capacity, in a softened and elastic state, would be very much greater, and a certain amount of food movement and digestion could be carried on. Before making an incision into the organ for the purpose of examining the contents, I soaked it in water; and then, when it was softened and relaxed, the internal space was much increased, and I could move these apparently packed stones upon each other.

I consider we have now sufficient data to accept it as a fact that these animals do, undoubtedly, swallow stones; the instance which I came across in the Zoological Gardens, and the testimony of several Cape gentlemen, must, I hold, be of convincing potency. You may very naturally ask me if there is any literature upon the subject, for in these days everything nearly gets into print; and in reply I must admit I can gain very little testimony from this source.

I have searched a great many books and a good number of records, and I have applied for information in likely quarters, but I have not much to offer. Mr. Wilson, the excellent curator of our museum, very kindly found a paragraph in the "Proceedings of the Zoological Society of London," for the year 1868, in which Mr. H. Brown writes a very long and interesting article, chiefly on the seals of Greenland; and on opening some of these animals stones and gravel were found in the stomach, whilst similar stones were found scattered about the spots on shore which the seals frequented. The stones, if small, could no doubt be passed through the intestinal tract; but if as large as in the specimen before us, they could not then be got rid of, and not even by vomiting, although the structure of the stomach would easily allow vomiting to occur. But I have no evidence that it does.

This habit of stone-swallowing has also been noted in a species of dolphin, the *Beluga catodon*, and I have also evidence that porpoises swallow them.

Mr. Bland Sutton, Lecturer on Comparative Anatomy at the Middlesex Hospital, and Pathologist to the London Zoological Society, informs me that in all sea-lions which have been examined by him after death a varying quantity of stones has been found in the stomach. He says, "In one case I saw two gallons of small rounded pebbles; but I have never seen them in the *Phocæ*."

Professor Lloyd Morgan, of Bristol University College, who lived at the Cape for some years, has kindly informed me that a friend of his has seen Cape seals toss up stones and catch them in their mouth.

With all this evidence before us, we cannot, I think, refuse to admit as a fact that the stones are swallowed by these creatures, and done so designedly. The stones could not get into the stomach accidentally, as from being entangled in or adhering to their food.

The next point, why? With what object they do it is not so easily answered. Are we willing to accept the pretty Cape legend? and if so, we must give seals credit for more than ordinary intelligence—chiefly instinctive in animals;

or may we look for some process which the stones subserve in the digestive economy, such as triturating the fish which are swallowed down almost whole? In this case they might correspond, in some measure, to the gizzard-stones of many birds. Or is the whole thing nothing more than a mischievous and playful habit?

When I examined the "ballast-bag" before us, a few days ago, I observed that all the stones I could see, without actually turning them out, were rounded and smooth, and so could easily move about in the stomach in its natural position and condition. Bearing this in mind, and thinking it very likely that these stones remain in these animals for a good length of time, I wrote to Mr. Sutton this question, "Did the stones you found in the sea-lions belong to the locality, say the London Gardens, or elsewhere, wherever the animals had been living at the time of death? or did they come from abroad?" His reply is very significant. "In all cases the stones found in the stomachs of the sea-lions were new to the locality. They must have been in the stomach in some cases for years. All were beautifully smooth and rounded."

Now with this evidence, imperfect as it is, before us, I think we must set aside the Cape legend, and come to look upon the habit of stone swallowing as one which has a true physiological basis.

Seals seize their prey greedily, and swallow it rapidly, their trenchant teeth not being adapted for much mastication; but the presence of a number of round smooth bodies in the stomach, such as these pebbles, would assist very materially in breaking up the food.

I have no better suggestion to offer at present anyhow, and I must therefore, whilst thanking you, ladies and gentlemen, for your patient hearing, leave the question in your hands.

Researches on Evaporation and Dissociation.

By Prof. WILLIAM RAMSAY, Ph.D., F.R.S., and
Prof. SYDNEY YOUNG, D.Sc.

THE investigations, a description of which is given in the following pages, were undertaken in order to arrive at a more accurate knowledge of the relations between the phenomena of the evaporation of stable solids and liquids, on the one hand, and of dissociating bodies, on the other. But as the relations of stable bodies as regards volume, temperature, and pressure had not been fully investigated, our experiments have been largely directed towards the elucidation of such relations. The question we have endeavoured to solve is:—What processes are actually in operation during the evaporation or volatilization of a liquid or solid?

(1) It was necessary first to prove that the dynamical method of measuring the vapour-pressures of solids gives results identical with the statical method. This has long been known to be the case with liquids. This led—

(2) To a proof of theoretical deductions by Kirchhoff, and subsequently and independently by James Thomson, that, at the same temperature, the pressure of vapour in contact with a substance in the solid state is lower than that of

vapour in contact with the liquid substance, at all temperatures below the melting-point of the solid. As the statical method in Regnault's hands appeared to negative the thermodynamical conclusion that the vapour-pressures of liquids and solids were different below the melting-point of the latter, the dynamical method was employed, which is, as a rule, capable of yielding more accurate results than the statical method.

(3) We next investigated the question regarding the vapour-pressures, or, to use the ordinary term, pressures of dissociation, of dissociable bodies. Do the statical and dynamical methods also give identical results, in measuring the vapour-pressures of dissociable bodies? To this the answer was,—In some cases they do, in the majority of cases they do not.

(4) It was found that our dynamical method of measuring vapour-pressures was applicable, with slight modifications, to liquids as well as to solids; and as our experiments on dissociable bodies had included measurements of the vapour-pressures of acetic acid, a description of the method, along with the results for acetic acid, was published in the *Trans. Chem. Soc.*, vol. xlvii., p. 42.

This was deemed necessary, on account of the contradictory results obtained by Regnault, Landolt, and Wüllner; and since Horstmann's views as regards the relation of acetic acid to ordinary dissociable bodies were based on Landolt's determinations of vapour-pressures, these views were proved to be incorrect.

(5) A method of obtaining constant known temperatures suggested itself, by using the vapours of liquids boiling under known pressures, these liquids being so chosen that their boiling-points overlapped. This necessitated careful determinations of the vapour-pressures of the following

liquids, chosen on account of their stability, and the ease with which they can be obtained in a state of purity: chlorobenzene, bromobenzene, aniline, methyl-salicylate, and bromonaphthalene. The vapour-pressures of mercury, as determined by Regnault, were at first taken to be correct; but it was found subsequently that they required redetermination.

(6) Comparison of the vapour-pressures of the liquids employed for maintaining constant temperatures led to the discovery of four approximate relations connecting the vapour-pressures of different substances; of these, two apply also to the ratio between the heat of vaporization and the increase of volume during the process of vaporization.

By means of these relations, the correctness of our measurements of the vapour-pressures of acetic acid was confirmed; and while these relations were proved to be applicable to all the substances investigated by Regnault in his classical researches, with the exception of mercury, and to the 28 ethers investigated by Schumann, as well as to oxygen and ethylene, for which data are furnished by Olzewski, the vapour-pressures of mercury and of the fatty acids (the latter determined by Landolt), proved the sole exceptions.

(7) An investigation of the vapour-pressures of mercury at high temperatures by ourselves, and of the fatty acids by Dr. Arthur Richardson, in this laboratory, proved that they also formed no exception to the general rule. These determinations also negative Kahlbaum's contention that the vapour-pressures of stable substances measured by the statical and the dynamical methods, are different.

(8) It was found that these relations hold with the vapour-pressures of solids, as well as of liquids; and in a separate paper, the data for solid and for liquid bromine and iodine

were furnished, and shown to form no exception. The limit of pressure to which these relations apply is about 5000 mms.; at higher pressures another term must be introduced in the equation.

So far, our work had to do with vapour-pressures; we next proceeded to determine, within wide limits of pressure and temperature, the relations of temperature, pressure, and volume in the cases of (9) methyl, ethyl, and propyl alcohols, ethyl ether and water; and (10) acetic acid. We have also made use of previous determinations by Andrews of these data for carbon dioxide, and by the Natansons for nitric peroxide.

We have also obtained the data for a mixture of two stable liquids, ethyl alcohol and ether, but those for a body which dissociates into two or more unlike molecules on rise of temperature are still wanting. These we hope to supply at some future time. These will supplement our knowledge, and we believe confirm our conclusions as regards the nature of liquids concerning which (11) we have drawn certain deductions from the behaviour of stable liquids contrasted with that of acetic acid and of nitric peroxide. From the measurements already made we are enabled (12) to draw conclusions as to the continuous passage of substances from the liquid to the gaseous state at all temperatures below and above their critical points.

These researches shall now be considered in order.

(1) The dynamical method of measuring the vapour-pressures of solids gives results identical with those obtained by the statical method (*Phil. Trans. of the Royal Society*, 1884, p. 37). Our experiments showed that at pressures below 4·6 mms. ice has definite temperatures of volatilization without melting, and for each pressure a definite temperature. In the dynamical method, the substance

boils or volatilizes at such a temperature that its vapour exerts a pressure just equal to the external pressure.

A modified Wollaston's cryophorus was used, with thermometers dipping into each bulb. Water which had been boiling for some time was placed in the bulbs, and boiled down, so that the steam expelled the air very completely from the apparatus. The opening through which the steam escaped was then closed. By suitable manipulation the bulb of one thermometer was coated with ice, not in contact with the sides of the outer bulb. When this bulb was surrounded by a hot bath, the other bulb being placed in a freezing mixture, the two thermometers registered the same temperature, but on admitting a minute quantity of air, the thermometer coated with ice showed a higher temperature than the thermometer in the condenser, by an amount which could be approximately calculated from the vapour-pressures of ice determined by Regnault, the air admitted adding its pressure to the vapour-pressure of the ice in contact with the sides of the condenser. Similar results were obtained with benzene and with acetic acid. Determinations of the vapour-pressures of camphor by both statical and dynamical methods were found to be completely concordant. For the statical determinations, the camphor was placed in a barometer-tube, with special precautions to exclude air, and the tube was jacketed with the vapours of pure liquids boiling under atmospheric pressure. In the accompanying figure, which represents the dynamical method, *A* represents the block of camphor round the thermometer *B*, inserted through an indiarubber cork into the tube *C*, jacketed by the wider tube *D*. *C* is connected with the condenser *E*, from which a tube *F* passes to the Sprengel's pump. Air could be admitted through a branch *G*, closed by an indiarubber tube and a screw-clip.

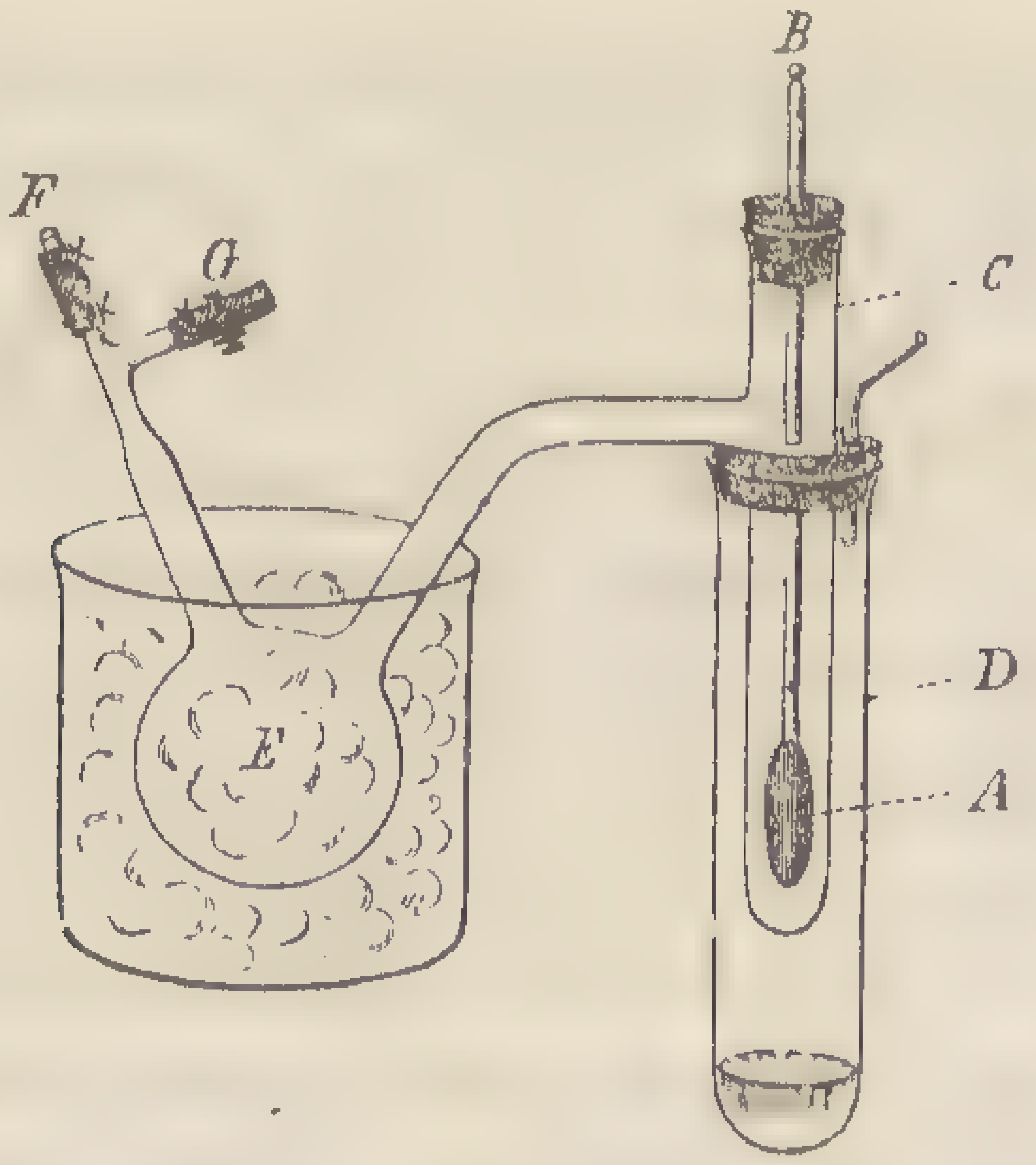

Fig. 1.

It appears from these experiments that the evaporation *at the surface* of a solid is capable of indefinite increase, however much heat the solid receives; and that the volatilizing-point of a solid rises with rise of pressure and falls with fall of pressure, as is the case with the boiling-point of a liquid; and moreover, that these temperatures are *sensibly* coincident with those corresponding to their vapour-pressures. That they cannot be *absolutely* identical is evident; for there must be a certain excess of pressure to produce a flow of vapour from the evaporating substance to the surrounding space, and consequently the evaporating substance must have a higher temperature, corresponding to the higher pressure in its immediate neighbourhood.

(2) The pressure of the vapour in contact with a substance in the solid state is lower than that of the vapour in contact with the liquid, at the same temperature for both, provided that temperature lies below the melting-point of the solid (*Phil. Trans.*, 1884, part ii., p. 461). This question was experimentally investigated by Regnault, with negative results (*Memoires*, xxvi., p. 751–759). Regnault says: "J'ai

constaté que la courbe construite sur ces expériences présentait une continuité parfaite avec celle que donnent les forces élastiques des vapeurs fournies par l'eau liquide aux températures supérieures à 0 degré." He again says, with reference to the other bodies examined: "En résumé, mes expériences prouvent que *le passage d'un corps de l'état solide à l'état liquide ne produit aucun changement appréciable dans la courbe des forces élastiques de sa vapeur; cette courbe conserve une parfaite régularité avant et après la transformation.*" The conclusion stated at the beginning of this paragraph was derived from thermodynamical considerations by Kirchhoff (*Pogg. Ann.*, vol. ciii.), and by James Thomson (*Phil. Mag.* (4), xlvii., p. 447), and Sir W. Thomson (*Trans. Roy. Soc., Edin.*, 1851, March 17th).

Our first experiments were with camphor, by the statical method. The pressures were found for many temperatures up to the melting-point (175°), and for liquid camphor up to 198°; and it is very evident on the diagram that the curves for liquid and for solid camphor meet at a re-entering angle at the melting-point. Similar results were obtained for benzene by the dynamical method. The apparatus shown in the wood-cut (Fig. 1) was employed, with the addition of a second tube passing through the cork, so arranged as to deliver benzene on to the bulb of the thermometer, which was covered with cotton-wool.

It was found possible to cool acetic acid far below its melting-point (16·7°), and keep it liquid, and a large number of good results were obtained. The curves meet at about 16·3°, and below the melting-point are obviously distinct, each being the result of numerous observations. Attempts made with the greatest care by the statical method gave, as with Regnault, no satisfactory results.

The next and last case taken in this paper is that of ice and water. Comparative results — *i.e.* boiling-points and volatilizing-points for water and ice at identical pressures — were obtained between 0° and $-5°$. Tables are also given of the volatilizing-points of ice, down to $-16°$, and these results, when compared with those which James Thomson obtained by recalculation of Regnault's data, are found to give differences of vapour-pressure of ice and water greater than his. But when the observed pressures for ice for temperatures below 0° were compared with the pressures calculated from a theoretical formula of Thomson's, it was found that the observed results agreed more nearly with those so calculated than with Regnault's results.

Fischer (*Wied. Ann.*, 1886, p. 400) has also published data for water and for benzene obtained by the statical method. His results with water agree very closely with ours; but he states that with benzene the vapour-pressure of the solid is not identical with that of the liquid at the melting-point. In a recent paper (*Phil. Mag.*, 1887, p. 61) we have pointed out that Fischer made use of the formula, $p = a + bt + ct^2$, which is not well adapted to represent the relations of temperature and pressure, in preference to the one suggested by Biot, $\log p = a + b\alpha^t (+ c\beta^t)$, which is better adapted for the purpose. Moreover Fischer did not make use of his experimental results at low temperatures in calculating the constants for his formula; if these errors be rectified, his results confirm the thermo-dynamical conclusion. We took this opportunity of redetermining the vapour-pressures of benzene, and for the liquid we obtained results identical with Fischer's; but for the solid somewhat higher temperatures were found for the same pressures; and we give reasons why we regard our results as more probable, based on experimental determinations of the heats of vaporization of solid and liquid benzene, on the heat of fusion of solid benzene, and on the specific heats of the solid and liquid. Our results have recently been the subject of a critique by R. v. Helmholtz (*Wied. Ann.*, N.F. 30, p. 401), which we regard as justified. In our critique on Fischer's results we pointed out that the numbers given as theoretical must be regarded as only approximate.

(3) Do the statical and dynamical methods of measuring vapour-pressure give identical results with dissociable bodies? (*Phil. Trans.*, 1886, part i., p. 71.) To answer this question, experiments were made with chloral hydrate, butyl-chloral hydrate, chloral methyl- and ethyl-alcoholates, ammonium carbamate, ammonium chloride, phthalic and succinic acids, aldehyde ammonia, metaldehyde, nitrogen peroxide, and acetic acid; besides chlorine hydrate and ethylamine hydrochloride, from which no results were obtained, and paraldehyde, which was found to be stable. As a rule, the methods already described were employed, but with nitrogen peroxide and ammonium chloride special methods were devised. It may be mentioned however that with ammonium carbamate and ammonium chloride, cylindrical blocks were cut out of large blocks of the salt, and were drilled with holes to fit the thermometer. Phthalic acid was dissolved in water, and the thermometer-bulb, covered with cotton-wool, was dipped repeatedly in the boiling aqueous solution, and then hung in a bell-jar over sulphuric acid for several days until dry. With nitrogen peroxide, asbestos was substituted for cotton-wool.

As the temperatures at which it was necessary to measure the vapour-pressures of ammonium chloride were so high that the sum of its vapour-pressure and that of mercury would have exceeded the limits to which the ordinary method is applicable, a piece of ammonium chloride was placed in the closed end of a U-tube, and kept in position by a constriction in the tube. The open end of the U-tube was connected with a gauge, the U-tube having been partially filled with mercury. The U-tube was jacketed with the vapour of mercury boiling under reduced pressures, so that its temperature was known. Air was first completely expelled from the closed end of the U-tube through the mercury by the vapour of the chloride. A large number of determinations was made by this method, and it was found in all cases that, after a certain time, the rate of increase of pressure at constant volume was constant, even for many hours. It was subsequently proved that this increase was due to slow action of hydrochloric acid gas on mercury (Than, *Annalen* 131, p. 131). Having ascertained the rate of increase, it was possible to calculate the true vapour-pressure for each temperature. It is worth noting here that the combination of dry HCl with dry NH_3 is very slow, even in presence of great excess of solid chloride.

With nitric peroxide, contact with mercury was inadmissible. A very large, thin-walled bulb, sealed to a graduated stem, was filled with mercury, and, in short, acted as an extremely delicate thermometer. It was surrounded by an outer, much thicker bulb, and the space between the two bulbs was filled with the vapour of peroxide; this space communicated with an external bulb at some distance containing a little liquid peroxide. Keeping the temperature of the pseudo-thermometer constant, by means of snow, any change of pressure caused by alteration of the temperature of the liquid nitrogen peroxide made the mercury rise or fall in the graduated stem. Experiments were made to ascertain to what pressures the readings on the graduated stem corresponded. For temperatures higher than 0°, the temperature of the pseudo-thermometer was kept constant by running water.

A few vapour-density determinations of some of these substances were also made by Hofmann's method.

The table on page 309 gives a summary of the results.

The behaviour of chloral hydrate deserves special notice, because the temperature of volatilization was perhaps not strictly independent of pressure, for at low pressures it was possible to heat the substance above its melting-point without melting; and the lower the pressure, the higher the temperature to which it could be raised. When the pressure was raised to about 60 mms. the temperature gradually fell, and on reaching the melting-point, 50·6°, the substance melted. It could again be caused to solidify by lowering the pressure at once, and the temperature then rose again.

It may also be noticed that it made no difference in the results with aldehyde ammonia and ammonium chloride, whether the pressure was raised by admission of air, or of either of the gaseous constituents of the dissociating compound.

It is evident that these substances may be divided into two groups: that in which the curves representing temperatures of volatilization and vapour-pressures are identical; and that in which these curves are distinct. The members of the first class behave like stable solids and liquids; and the class includes the three substances, ammonium chloride, nitrogen peroxide, and acetic acid. With the first of these, dissociation is nearly complete 60° below the temperature of volatilization at normal pressure; with the second, dissociation amounts to less than 20 p.c. at its boiling-point; while the dissociation of acetic acid rests on indirect evidence.

The second group, which contains the rest of these substances, may be divided into two sections: that in which

Name of substance.	Vapour density shows—	Temperature of volatilization.	Vapour-pressure, or pressure of dissociation.
Chloral hydrate	Dissociation almost complete at 78°	Independent of pressure .	Curve of ordinary form
Butyl chloral hydrate . .	Complete dissociation at 160° .	do.	do.
Chloral methyl-alcoholate	At 78·5°, about 78 p.c. dissociated	do.	do.
Chloral ethyl-alcoholate .	At 78°, 82·5 to 83 p.c. dissociated	do.	do.
Ammonium carbamate .	Total dissociation	Constant at about 65° under all pressures . .	do.
Ammonium chloride . .	Dissociation very nearly complete at 280°	(Curves	identical)
Phthalic acid	Vapour density not determined; dissociation probably very large. No constant melting-point	Rudimentary curve at low pressures	Abrupt change of direction of curve
Succinic acid	Vapour density not determined; dissociation probably less complete than with phthalic acid; constant melting-point . . .	Curve more obvious than with phthalic acid	Appears to depend on amount of substance present. Curve shows double flexure
Aldehyde-ammonia . . .	70 p.c. dissociated at 78·25, under reduced pressure . . .	Curve of usual form . .	Curve of usual form; but pressure higher than by other method
Paraldehyde	No dissociation	(Curves	identical)
Metaldehyde	(Not	determined)	Equilibrium established only after very prolonged heating
Nitrogen peroxide . . .	Dissociation small below 20° . .	(Curves	identical)
Acetic acid	Dissociation of $C_4H_8O_4$ at low temperatures, small	(Curves	identical)
Ethylamine hydrochloride, and chlorine hydrate	No results		

pressure has no apparent influence on the temperature of volatilization; and that in which its influence is partial.

Judging from the determinations of vapour-density which have been made, it may fairly be stated that those substances which show the greatest amount of dissociation show also the greatest divergence between the curves, and the least connection between temperature of volatilization and pressure.

If a substance is capable of existing in the gaseous state with only partial dissociation, it exerts pressure which is the sum of the vapour-pressure of the undecomposed substance, and of the bodies resulting from its dissociation. If these two pressures could be determined separately by experiment for a series of temperatures, we should have two separate curves, the resultant of which would be that representing the pressure determined by the barometer-tube method. Now the combination of two such curves might yield a curve of double flexure, as shown in our original memoir (*loc. cit.* p. 120); and an indication of this may possibly be seen with succinic acid. Or it may give a curve with an abrupt change of direction, and phthalic acid may afford an example of this. Or, lastly, the curve may be indistinguishable from an ordinary vapour-pressure curve, as is the case with most substances.

It may be also noticed that a dissociable solid, evaporating from a free surface, undergoes no fractionation, the residue having always the original composition; while a dissociable liquid undergoes fractionation.

4. *Vapour-pressures of acetic acid—correction of former results* (*Trans. Chem. Soc.*, 1885, p. 42; and 1886, p. 805).—The following table summarizes our results, and contrasts them with those of Regnault (R.), Landolt (L.), and Wüllner (W.).

Temperature.	R. and Y.		R.	L.	W.
	Solid.	*Liquid.*			
	mms.	*mms.*	*mms.*	*mms.*	*mms.*
0°	2·02	3·30	3·23 to 4·89	7·6	...
10	5·19	6·38	6·30 to 8·20	12·1	...
20	...	11·73	11·58 to 13·65	18·9	19·0
30	...	20·61	...	29·1	30·5
40	...	34·77	...	44·1	45·5
50	...	56·56	...	66·0	72·0
60	...	88·94	...	97·4	107·3
70	...	136·0	...	142·0	155·2
80	...	202·3	...	204·3	232·9
90	...	293·7	...	290·6	346·7
100	...	417·1	...	408·5	473·0
110	...	580·8	...	...	...
120	...	791·0	...	...	...

Results are also given by Bineau, at 15°, 7·70 mms.; at 22°, 14·5 mms.; at 32°, 23 mms.; and by Naumann, at 78°, 185 mms.

It will be seen that Landolt's and Wüllner's measurements show little concordance with each other, and less with those by Regnault, Bineau, Naumann, and ourselves.

5. *Method of obtaining constant known temperatures* (*Trans. Chem. Soc.*, 1885, p. 640).—In the following table, which it is desirable to reproduce in full, the temperatures are those of an air thermometer, and the pressures are in mms. of mercury at 0°. It is necessary in using the method of jacketing with vapours to correct these pressures to mms. of mercury at the temperature of the room, which can be done graphically with sufficient accuracy. The determinations of the vapour-pressures of carbon disulphide and ethyl alcohol are Regnault's. The rest are our own.

Carbon Disulphide.—Range from 0° to 50°.

T.	P.	T.	P.	T.	P.	T.	P.
0°	127·9	13°	224·95	26°	374·95	39°	596·85
1	133·85	14	234·4	27	389·2	40	617·5
2	140·05	15	244·15	28	403·9	41	638·7
3	146·45	16	254·25	29	419·0	42	660·5
4	153·1	17	264·65	30	434·6	43	682·9
5	160·0	18	275·4	31	450·65	44	705·9
6	167·15	19	286·55	32	467·15	45	729·5
7	174·6	20	298·05	33	484·15	46	753·75
8	182·25	21	309·9	34	501·65	47	778·6
9	190·2	22	322·1	35	519·65	48	804·1
10	198·45	23	334·7	36	538·15	49	830·25
11	207·0	24	347·7	37	557·15	50	857·1
12	215·8	25	361·1	38	576·75		

Ethyl Alcohol.—Range from 40° to 79°.

T.	P.	T.	P.	T.	P.	T.	P.
40°	133·7	50°	220·0	60°	350·3	70°	541·2
41	140·75	51	230·8	61	366·4	71	564·35
42	148·1	52	242·05	62	383·1	72	588·35
43	155·8	53	253·8	63	400·4	73	613·2
44	163·8	54	265·9	64	418·35	74	638·95
45	172·2	55	278·6	65	437·0	75	665·55
46	181·0	56	291·85	66	456·35	76	693·1
47	190·1	57	305·65	67	476·45	77	721·55
48	199·65	58	319·95	68	497·25	78	751·0
49	209·6	59	334·85	69	518·85	79	781·45

Chlorobenzene.—Range from 70° to 132°.

T.	P.	T.	P.	T.	P.	T.	P.
70°	97·9	86°	181·7	102°	312·5	118°	512·05
71	101·95	87	187·3	103	322·8	119	527·25
72	106·1	88	194·1	104	333·35	120	542·8
73	110·41	89	201·15	105	344·15	121	558·7
74	114·85	90	208·35	106	355·25	122	575·05
75	119·45	91	215·8	107	366·65	123	591·7
76	124·2	92	223·45	108	378·3	124	608·75
77	129·1	93	231·3	109	390·25	125	626·15
78	134·15	94	239·35	110	402·55	126	643·95
79	139·4	95	247·7	111	415·1	127	662·15
80	144·8	96	256·2	112	427·95	128	680·75
81	150·3	97	265·0	113	441·15	129	699·65
82	156·05	98	274·0	114	454·65	130	718·95
83	161·95	99	283·25	115	468·5	131	738·65
84	168·0	100	292·75	116	482·65	132	758·8
85	174·25	101	302·5	117	497·2		

Bromobenzene.—Range from 120° to 157°.

T.	P.	T.	P.	T.	P.	T.	P.
120°	274·9	130°	372·65	140°	495·8	150°	649·05
121	283·65	131	383·75	141	509·7	151	666·25
122	292·6	132	395·1	142	523·9	152	683·8
123	301·75	133	406·7	143	538·4	153	701·65
124	311·15	134	418·6	144	553·2	154	719·95
125	320·8	135	430·75	145	568·35	155	738·55
126	330·7	136	443·2	146	583·85	156	757·55
127	340·8	137	455·9	147	599·65	157	776·95
128	351·15	138	468·9	148	615·75		
129	361·8	139	482·2	149	632·25		

Aniline.—Range from 150° to 185°.

T.	P.	T.	P.	T.	P.	T.	P.
150°	283·7	159°	374·6	168°	487·25	177°	625·05
151	292·8	160	386·0	169	501·25	178	642·05
152	302·15	161	397·65	170	515·6	179	659·45
153	311·75	162	409·6	171	530·2	180	677·15
154	321·6	163	421·8	172	545·2	181	695·3
155	331·7	164	434·3	173	560·45	182	713·75
156	342·05	165	447·1	174	576·1	183	732·65
157	352·65	166	460·2	175	592·05	184	751·9
158	363·5	167	473·6	176	608·35	185	771·5

Methyl Salicylate.—Range from 175° to 224°.

T.	P.	T.	P.	T.	P.	T.	P.
175°	215·1	188°	313·05	201°	443·75	214°	615·05
176	221·65	189	321·85	202	455·35	215	630·15
177	228·3	190	330·85	203	467·25	216	645·55
178	235·15	191	340·05	204	479·35	217	661·25
179	242·15	192	349·45	205	491·7	218	677·25
180	249·35	193	359·05	206	504·35	219	693·6
181	256·7	194	368·85	207	517·25	220	710·1
182	264·2	195	378·9	208	530·4	221	727·05
183	271·9	196	389·15	209	543·8	222	744·35
184	279·75	197	399·6	210	557·5	223	761·9
185	287·8	198	410·3	211	571·45	224	779·85
186	296·0	199	421·2	212	585·7		
187	304·45	200	432·35	213	600·25		

Bromonapthalene.—Range from 215° to 281°.

T.	P.	T.	P.	T.	P.	T.	P.
215°	158·85	232°	248·3	249°	377·3	266°	557·6
216	163·25	233	254·65	250	386·35	267	570·05
217	167·7	234	261·2	251	395·6	268	582·7
218	172·3	235	267·85	252	405·05	269	595·6
219	176·95	236	274·65	253	414·65	270	608·75
220	181·75	237	281·6	254	424·45	271	622·1
221	186·65	238	288·7	255	434·45	272	635·7
222	191·65	239	295·95	256	444·65	273	649·5
223	196·75	240	303·35	257	455·0	274	663·55
224	202·0	241	300·9	258	465·6	275	677·85
225	207·35	242	318·65	259	476·35	276	692·4
226	212·8	243	326·5	260	487·35	277	707·15
227	218·4	244	334·55	261	498·55	278	722·15
228	224·15	245	342·75	262	509·9	279	737·45
229	230·0	246	351·1	263	521·5	280	752·95
230	235·95	247	359·65	264	533·35	281	768·7
231	242·05	248	368·4	265	545·35		

Mercury.—Range from 270° to 359°.

T.	P.	T.	P.	T.	P.	T.	P.
270°	123·92	293°	211·76	316°	344·81	339°	538·56
271	126·97	294	216·50	317	351·85	340	548·64
272	130·08	295	221·33	318	359·00	341	558·87
273	133·26	296	226·25	319	366·28	342	569·25
274	136·50	297	231·25	320	373·67	343	579·78
275	139·81	298	236·34	321	381·18	344	590·48
276	143·18	299	241·53	322	388·81	345	601·33
277	146·61	300	246·81	323	396·56	346	612·34
278	150·12	301	252·18	324	404·43	347	623·51
279	153·70	302	257·65	325	412·43	348	634·85
280	157·35	303	263·21	326	420·58	349	646·36
281	161·07	304	268·87	327	428·83	350	658·03
282	164·86	305	274·63	328	437·22	351	669·86
283	168·73	306	280·48	329	445·75	352	681·86
284	172·67	307	286·43	330	454·41	353	694·04
285	176·79	308	292·49	331	463·20	354	706·40
286	180·88	309	298·66	332	472·12	355	718·94
287	185·05	310	304·93	333	481·19	356	731·65
288	189·30	311	311·30	334	490·40	357	744·54
289	193·63	312	317·78	335	499·74	358	757·61
290	198·04	313	324·37	336	509·22	359	770·87
291	202·53	314	331·08	337	518·85		
292	207·10	315	337·89	338	528·63		

6. *Relations between the heats of vaporization, and between the vapour-pressures of different substances. (Phil. Mag.,*

1885, p. 515; 1886, pp. 33, 135 (vol. i.), p. 32 (vol. ii.)—In the thermodynamical equation,

$$\frac{L}{s_1-s_2}=\frac{dp}{dT}\cdot\frac{T}{J}.$$

(L=heat of vaporization: s_1=vol. of unit mass of saturated vapour; s_2=vol. of unit mass of liquid; $\frac{dp}{dT}$=rate of increase of pressure per unit rise of temperature; J=mechanical equivalent of heat), the following relations hold: (1) The amount of heat required to produce unit increase of volume in the passage from the liquid to the gaseous state, at the boiling point under ordinary pressure, is approximately constant for all bodies, or $\frac{L}{s_1-s_2}=c$. (2) If the amounts of heat required to produce unit increase of volume in the passage from the liquid to the gaseous state be compared at different pressures for any two bodies, then the ratio of the amount at the boiling-point under pressure p_1, to the amount at another pressure p_2, is approximately constant for all liquids. It follows, that the external and total work bear an approximately constant ratio to each other at any one pressure, whatever be the liquid. (3) The values of $\frac{dp}{dT}T$ are approximately the same for all stable bodies at the same pressure; but the differences are real, and are not due to errors of experiment or of calculation. (4) The rate of increase of this value, $\frac{dp}{dT}T$, with rise of pressure, is the same for all stable bodies, at any rate for pressures between 150 mms. and 2,000 mms. The first and third, and the second and fourth of these laws are identical, but the proof rests on an entirely different experimental basis, and is much more complete for the pressures than for the heats of vaporization. (5) A relation exists between the ratios of the absolute temperatures of all bodies, whether solid or liquid, which may be expressed in the case of any two bodies by the equation $R'=R+c(t'-t)$, where R is the ratio of the absolute temperatures of the two bodies corresponding to any vapour-pressure, the same for both; R', the ratio at any other pressure, again the same for both; c is a constant, which may be 0 or a small plus or minus number, and t' and t the temperatures of one of the bodies, corresponding to the two vapour-pressures. It may be noticed that the equation $R'=R+c(t'-t)$ is not symmetrical, in as much as t' and t may refer to either of the substances compared; but the difference, within 5,000 mms. is small, and may be neglected. When $c=0$, $R'=R$, or the ratio of the absolute temperatures is a constant at all pressures; and this is the case with chloro- and bromo-benzene, ethyl chloride and bromide, and with the 28 ethers investigated by Schumann. If the vapour-pressures of a substance in the liquid and the solid states be compared with those of a third substance, the sign of c (+ or —) for the solid is always contrary to that for the liquid. This has been proved for acetic acid and for bromine and iodine (*Trans. Chem. Soc.*, 1886, p. 453). (6) For higher pressures than 5,000 mms., the more complex equation, $R'=R+c(t'-t)+c'(t'-t)^2$ gives more accurate results. (7) The equation $R'=R+c(t'-t)$ holds where R' and R represent the ratios of the products $\frac{dp}{dT}T$ (instead of the ratios of the absolute temperatures) for any two substances at the same pressures.

Space will not permit of a proof of these relations; for details we must refer to the original papers.

7. *Vapour pressures of mercury.* (*Trans. Chem. Soc.*, 1886, p. 37).—The vapour pressures of mercury were previously

determined by Regnault (*Mémoires de l'Académie*, vol. 21, pp. 230, 502; vol. 26, p. 520). His results appear to have been regarded by himself as possessing no great claim to accuracy, and quotations are adduced in the original paper to this effect. If the relation $R' = R + c(t' - t)$ is true, then it is necessary only to determine the vapour-pressures at a few widely different temperatures, and to compare them with some standard substance. We have in this case chosen water. The data are (1), determinations by ourselves at 222·15°, 270·3°, and 280·2°, by an arrangement similar to that described in the paragraph on the dissociation of ammonium chloride (p. 306), but with the space at the closed end of the U-tube containing only mercury-vapour. The jacketing vapours were methyl-salicylate and bromonaphthalene. (2) Regnault's own determinations of the boiling-point of mercury under atmospheric pressure, which are not very concordant; the mean however cannot be far wrong. (3) Two determinations at the boiling-point of sulphur. In this method, the mercury-vapour, generated in a small bulb, which was heated by the vapour of boiling sulphur, exerted its pressure on a column of mercury confining air, kept at a constant temperature. The pressure was calculated from the alteration of volume of the air. The results are shown in the following table:

Temperature Centigrade.	Temperature Absolute.	Pressure.	Absolute temperature of water at pressure p.	Ratio of absolute temp. of mercury and water at pressure p.
222·15°	495·15°	34·4	304·5°	1·6262
270·3	543·3	124·35	329·2	1·6504
280·2	553·2	157·15	334·2	1·6553
358·46	631·46	769·93	373·36	1·6913
357·48	630·48	768·58	373·27	1·6890
359·27	632·27	768·10	373·25	1·6940
358·68	631·68	760·83	373·03	1·6934
447·0	720·0	2896·9	415·26	1·7338
448·0	721·0	2904·5	415·36	1·7359

The value of c, if the temperatures of mercury are chosen as ordinates, is 0·0004788; if those of water are chosen, 0·0009792. It was more convenient to employ the constant derived from the absolute temperatures of mercury. The method of calculation was as follows: A diagram was constructed, on which the ordinates were the absolute temperatures of mercury, and the abscissæ the ratios of absolute temperatures of mercury and water, at pressures corresponding to the absolute temperatures of mercury. From the equation $R'=R+c(t'-t)$, it is evident that the points must lie in a straight line. A point was read, giving the ratio at any one temperature; the absolute temperature of water was calculated from the ratio; the vapour-pressure of water corresponding to this temperature is the same as that of mercury, inasmuch as the ratios refer to equal pressures. Thus at an absolute temperature of mercury of 508°, the ratio, as read from the line, was 1·6331. The absolute temperature of water was therefore $\frac{508^\circ}{1 \cdot 6331}=311 \cdot 06^\circ$. The vapour-pressure of water at 311·06°, ascertained from Regnault's tables, is 49·466 mms. This is therefore the vapour-pressure of mercury at an absolute temperature of 508°. The ratios corresponding to other absolute temperatures of mercury were calculated from the equation $R=R'+c(t'-t)$, the value of R being 1·6331 when $t=508^\circ$, as given above. As the vapour-pressures of water are uncertain below a pressure of 4·6 mms., it was necessary to calculate the constants for a formula of the form $\log p=a+b\,\alpha^t$. We append a table of comparison of results by different experimenters; the vapour-pressures for each degree at higher temperatures have been already given on p. 314.

Temp.	Regnault. *mms.*	Hagen. *mms.*	Hertz. *mms.*	R. and Y. *mms.*
0°	0·02	0·015	0·00019	
10	0·0268	0·018	0·00050	
20	0·0372	0·021	0·0013	
30	0·0530	0·026	0·0029	
40	0·0767	0·033	0·0063	0·008
50	0·1120	0·042	0·013	0·015
60	0·1613	0·055	0·026	0·029
70	0·2410	0·074	0·050	0·052
80	0·3528	0·102	0·093	0·093
90	0·5142	0·144	0·165	0·160
100	0·7455	0·210	0·285	0·270
120	1·5341		0·779	0·719
140	3·0592		1·93	1·763
160	5·9002		4·38	4·013
180	11·000		9·23	8·535
200	19·90		18·25	17·015
220	34·70		34·90	31·957

8. Determinations of the vapour-pressures of liquid and solid iodine and bromine (*Trans. Chem. Soc.*, 1886, p. 453) confirmed our results as to the difference of the vapour-pressures of a substance in the solid and liquid state at the same temperatures; and also the relations described under § 6. From this research it appears that the boiling-points of bromine and iodine under normal pressure are respectively 58·7° and 184·35°; and the melting-points, − 7·05° and 114·15°. The melting-pressures,—*i.e.* the vapour-pressure of solid or liquid at the melting-point,—are for bromine, 44·5 mms., and for iodine 90 to 91 mms.

9. The subsequent researches have reference to the relations of temperature, pressure, and volume of methyl- ethyl-, and propyl-alcohols, ether, water, acetic acid, and a mixture of alcohol and ether.

The vapour-pressures at low temperatures were measured by our dynamical method. Those at high temperatures, up to the respective critical points, by a modification of Andrews' apparatus, the tube containing the substance being jacketed by vapours, as already described, and the pressures being registered by an air-gauge, corrected by means of Amagat's recent determinations of the compressibility of air. The constants for formulæ of the type $\log p = a + b\alpha^t + c\beta^t$ are :

	For Methyl Alcohol.	For Ethyl Alcohol.	For Ether.
a	22·307096	5·072030	5·9834771
$\log b$	1·2619587	$\bar{2}$·6406131	0·5240258
$\log c$	0·3855770	0·6050854	$\bar{1}$·5733238
$\log \alpha$	$\bar{1}$·99988416	0·00337538	$\bar{1}$·99827459
$\log \beta$	$\bar{1}$·99599796	$\bar{1}$·99682124	$\bar{1}$·99130336
	b and c are both negative.	c is negative.	b and c are both negative.

	For Propyl Alcohol.	For Acetic Acid.
a	4·479370	6·700311
$\log b$	$\bar{1}$·3915059	0·6879733
$\log c$	0·5509601	0·1162143
$\log \alpha$	0·001641423	$\bar{1}$·99881514
$\log \beta$	$\bar{1}$·99657025	$\bar{1}$·99450874
	c is negative: $t = t^{\circ}C - 20$	b and c are both negative.

The values of $\frac{dp}{dt}$, which were required in calculating the heats of vaporization, were ascertained by calculating the pressure one-tenth of a degree above and below the required temperature, and multiplying the difference by 5.

The volume occupied by 1 gram. of these substances in the gaseous state was determined for low temperatures and pressures by means of a modification of Hofmann's apparatus, in which pressure volume and temperature could be altered at will. At higher temperatures and pressures similar relations were determined by means of the modified Andrews' apparatus. With methyl alcohol the limit of temperature was 240°; with ethyl alcohol, 246°; and with the other substances, 280°. For a description of the methods and apparatus the memoir on ether must be consulted. As the data are very voluminous, we cannot give them here, even in abstract; but we may mention that they comprise—determinations of the compressibility of ethyl and propyl alcohol, ether, and water, both in the liquid and in the gaseous states; the volumes of all the liquids at pressures equal to their vapour-pressures at the temperatures of measurement, or "orthobaric" volumes of liquid; the volumes of the saturated vapour, or "orthobaric" volumes of gas; from these the densities of the unsaturated and saturated vapours (H at $t°$ and p mms. $=1$), within wide limits, were calculated. The heats of vaporization were then calculated for definite temperatures by means of the thermodynamical equation, $\frac{L}{s_1 - s_2} = \frac{dp}{dT} \frac{T}{J}$. It may be here mentioned that the apparent critical temperatures and pressures are for methyl alcohol, 240·0° and 59,700 mms.; for ethyl alcohol, 243·1° and 47,650 mms.; for propyl alcohol, 263·64° and 38,120 mms.; and for ether, 193·8° and 27,080 mms.

10. The data for acetic acid are given in the *Trans. Chem. Soc.*, 1886, p. 790. The orthobaric volumes of the liquid are determined between 13° and 280°; the vapour-pressures between $-5°$ and 280°; the densities of the vapour, saturated and unsaturated, between 50° and 280°. The heats of vaporization were also calculated.

11. *The Nature of Liquids* (*Phil. Mag.*, 1887, p. 129).—The striking analogy between the evaporation of liquids and the dissociation of chemical compounds has led Naumann and others to suggest as probable that the molecules of a liquid consist of congeries of gaseous molecules, and present analogy with the molecules of a compound body; differing however in this respect, that while the molecules of compounds result, as a rule, from the combination of unlike

gaseous molecules, those of stable liquids consist of like molecules. The opposite view is taken by Van der Waals, and the question is well stated in the preface to his work on the *Continuität des gasförmigen u. flüssigen Zustandes:* "Streng genommen, habe ich noch mehr beweisen wollen, nämlich die Identität beider Aggregatzustände. Findet nämlich die schon zum Theil begründete Vermuthung, dass auch im flüssigen Zustand, die Moleküle nicht zusammenfallen um grössere Atomcomplexe zu bilden ihre volle Bestätigung, so gibt es zwischen den beiden Zuständen nur noch den Unterschied der grösseren oder kleineren Dichte; mithin, nur einen quantitativen Unterschied." The supposition that the complexity of the molecules is different in the two states, is, we consider, negatived by the following considerations:—The vapour-density of a gas may become abnormally high by either or both of two causes: the existence of complex molecules (congeries of gaseous molecules in the gaseous state); or by the greater propinquity of the molecules of gas, caused by their mutual attraction. We found with alcohol, that the density of the saturated vapour was normal at temperatures below 40° or 50°, and remained normal down to 13°, the lowest temperature at which observations could be made. With ether the vapour-density was approaching normality at 13°, and from the form of the curve would have doubtless become normal at a lower temperature. In both cases, with increase of temperature, and corresponding increase of pressure, the density of the saturated vapour increased towards the critical point with accelerating rapidity, until, at the critical point, the mass of unit volume of the saturated vapour was equal to that of the liquid (Fig. 2).

At the critical point the heat of vaporization of a stable liquid is theoretically zero; below that temperature we found it to increase with alcohol and with ether, as the temperature fell. With ether the increase was found to be continuous to the lowest observed temperature, 13°; whereas with alcohol it becomes practically constant below about 20° (Fig. 3). Our calculated numbers correspond well with direct measurements by various observers, at the boil-

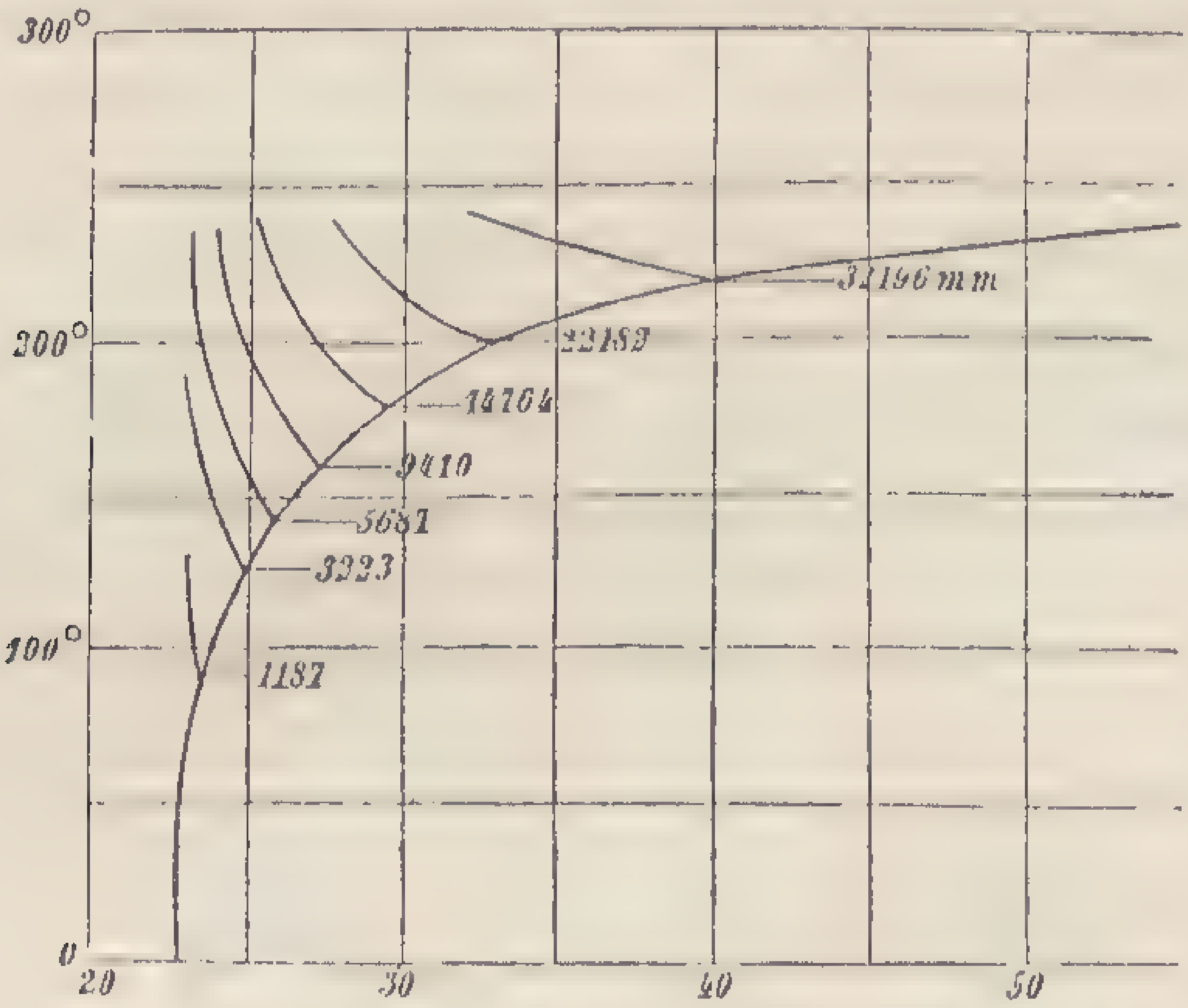

Fig. 2. Vapour Densities of Alcohol (H = 1 at t° and p mm.).

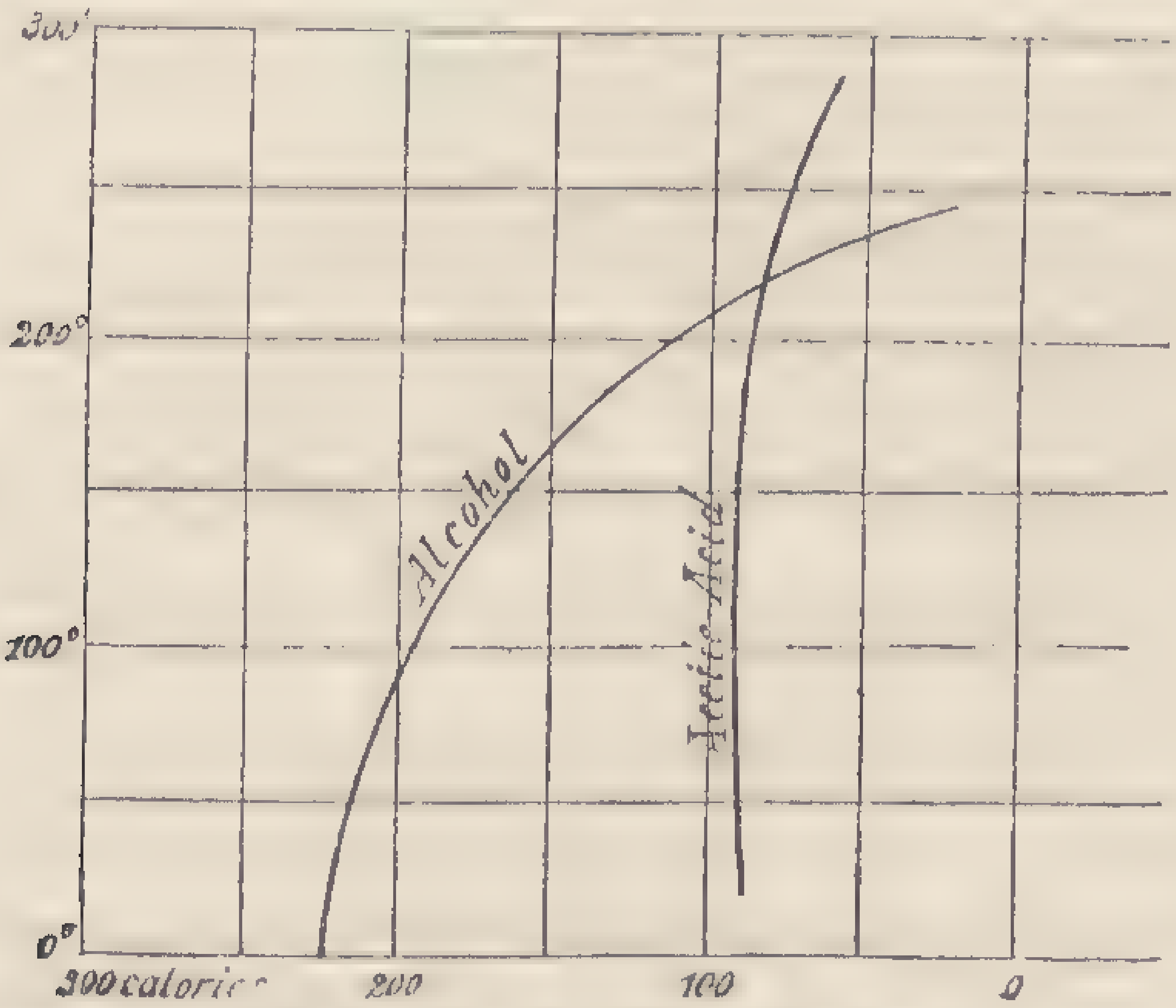

Fig. 3. Heats of Vaporization.

ing-points under atmospheric pressure. With methyl and propyl alcohols similar results were obtained.

With acetic acid the results were very different. On raising the temperature above 150°, the density of the saturated vapour increased as with other liquids; but below that temperature (at which the vapour-density was 50·06, the calculated density being 30) the vapour-density, instead of continuing to fall, rose more and more rapidly with fall of temperature, until at 20° it was approximately 59, and apparently, from the form of the curve, was continuing to rise more and more rapidly with fall of temperature (Fig. 4). It may be mentioned that direct observations by Bineau give nearly the same value.

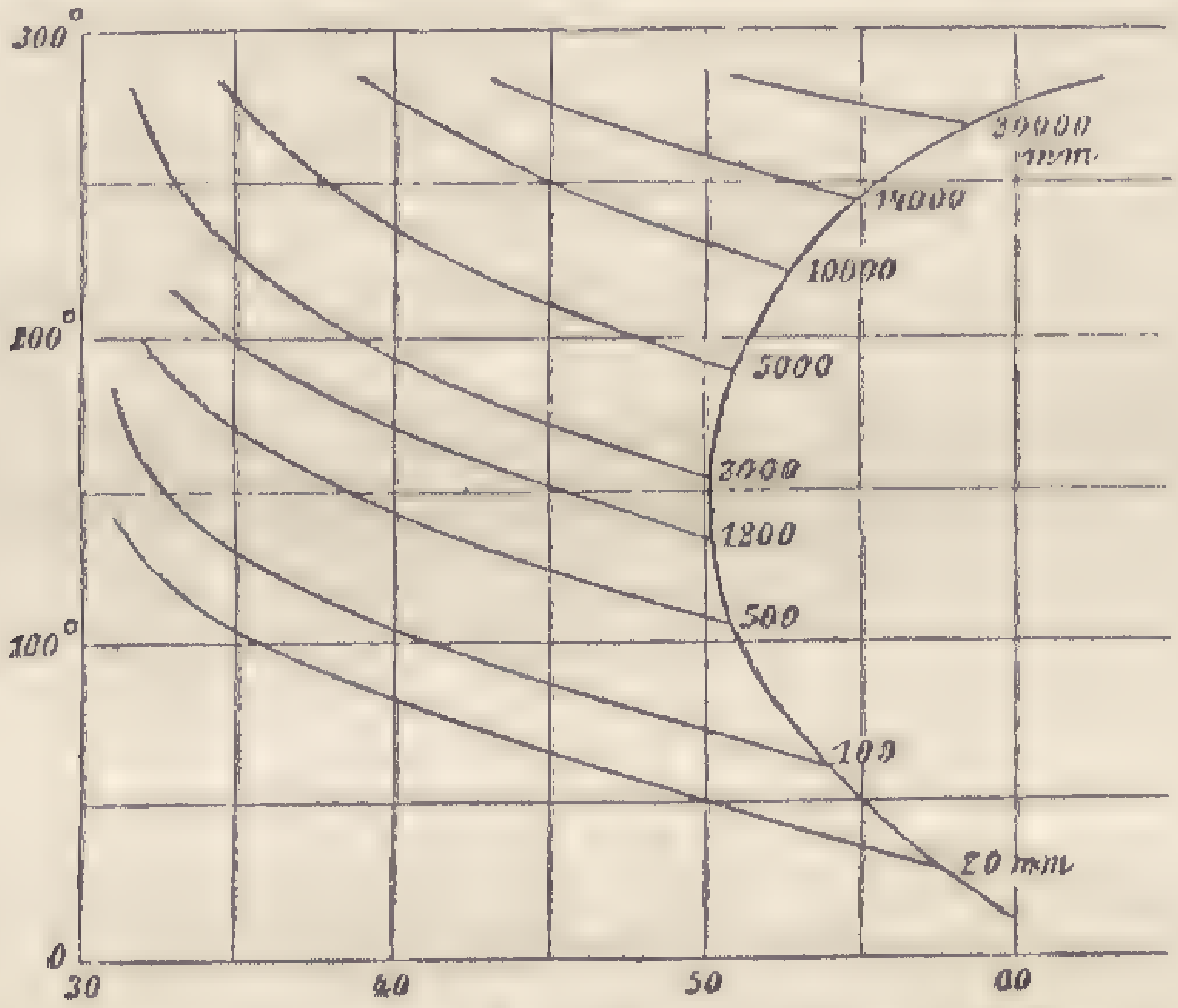

Fig. 4. Vapour Densities of Acetic Acid (H = 1 at t° and p mm.).

The curve representing the heats of vaporization of acetic acid at various temperatures also differs entirely in form from those of the alcohols and ether; for it exhibits a maximum at 110°, and decreases both with rise and with fall of temperature (Fig. 3). It is difficult to draw any conclusion from a comparison of our measurements of this quantity at the boiling-point under atmospheric pressure with those of other observers; but it may be stated that our result differs far less from the observation of Favre and Silbermann than theirs does from that of Berthelot.

Messrs. E. and L. Natanson have recently published a research on the vapour-densities of nitric peroxide (NO_2 or N_2O_4), which, taken in conjunction with experiments of ours on the vapour-pressures of that body, show that nitric peroxide behaves like acetic acid, the density of its saturated vapour rising with fall of temperature, but in their experiments never exceeding that of N_2O_4.

It appears to us that these results negative the chemical explanation of the constitution of liquids; or, to confine ourselves to known cases, of the alcohols and of ether. The molecules of these liquids cannot, we think, be regarded as complex, consisting of gaseous molecules in chemical combination with each other, as, for example, $n\,C_2H_6O$, where n is any definite number. We believe rather that the physical explanation of the nature of liquids is the correct one, and that the differences between liquids and gases lies merely in the relative proximity of their molecules. The chief argument for this view is that it is difficult to conceive that the rise of vapour-density of acetic acid both at high and at low temperatures can be produced by the same cause under conditions so radically different; for at high temperatures we have conditions unfavourable to chemical combination, but, owing to the necessarily high pressure, the molecules are in close proximity; whereas at low temperatures, the conditions are favourable to chemical combination, while the molecules, owing to the corresponding low pressures, are very far apart. The increase in the density of the saturated vapour of acetic acid at low temperatures is therefore, in all probability due to chemical combination between the simple molecules, while the increase at high temperatures is due to a general attraction (cohesion) of the molecules for each other. In other words, the cause of abnormality at low temperatures is a chemical one; at high temperatures, a physical. Now the increase of the density of the saturated vapour at high temperatures is common to all bodies; but the increase at low temperatures is only exhibited by such bodies as acetic acid and nitrogen peroxide, which, there is every reason to believe, undergo dissociation. Its absence in the case of the alcohols and ether affords a strong argument against the existence of chemical combination between the simple molecules. But it might be asserted that in the passage from the gaseous to the liquid state combination occurs. That this cannot be the case is evident from a consideration of the behaviour of liquids near their critical points. For the specific volumes of liquid and gas, just below the critical point, are nearly equal; and were the liquid to consist of congeries of gaseous molecules, there would necessarily be fewer molecules in unit volume of the liquid, than in unit volume of the gas,—an improbable conception.

Horstmann has also made experiments on the vapour-densities of acetic acid. In his first paper (*Berichte*, 2, p. 299), he suggested that a difference should exist between the forms of the curves representing the densities of the saturated vapour in the case of a true dissociating substance, and of acetic acid. He supposed that the vapour-density of dissociating substances increases with fall of temperature, but that in the case of acetic acid it decreases; and in his second paper (*Berichte*, 3, p. 78), he gives experimental determinations of the density of the saturated vapour, which, using his calculations, confirm his view, although they are in direct contradiction to the experimental evidence adduced by Bineau (*Annalen*, 60, p. 157) and by us. But in his calculations, he employed Landolt's vapour-pressures, which, at the lowest temperatures, are nearly double those obtained by Regnault and by us. On recalculating his results with true vapour-pressures, it is found that there is a tendency for the density of the saturated vapour to rise, instead of to fall, with decrease of temperature, though the results are not sufficiently concordant to be plotted in the form of a curve. (For details, see *Trans. Chem. Soc.*, 1886, p. 809.)

Whereas, with the alcohols, and with ether, as soon as condensation from gas to liquid occurs, pressure remains constant until condensation is complete, with acetic acid, and with chloral ethyl-alcoholate the pressure continues to rise, implying the condensation of the more complex molecules before the simpler ones; in fact, the substance behaves like the mixture of alcohol and ether, of which it was impossible to determine the true vapour-pressures.

A somewhat similar phenomenon was observed with water, but in this case it was most noticeable before visible condensation took place, and it was proved to be due to the hygroscopic nature of the glass. Owing to this source of error it was impossible to obtain direct determinations of the density of the saturated vapour of water, but the accuracy of the determinations of vapour-pressures was not affected.

Many attempts have been made to discover an equation which shall represent accurately the relations of temperature, pressure, and volume of a substance in both the liquid and gaseous state. The most important advance was made by Van der Waals, who proposed the equation $\left(p+\frac{a}{v^2}\right)\left(v-b\right)=\mathrm{R}\left(1+\alpha t\right)$ in place of the ordinary equation for a perfect gas, $pv = R(1 + \alpha t)$. But neither the equation as proposed by Van der Waals, nor as modified by Clausius, Sarrau, and others, give results which are more than approximately correct; indeed, at very high pressures, the deviations become very large.

A step in this direction has been made by the discovery of a simple relation between the pressure and temperature of a substance in either the liquid or gaseous state at constant volume, which may be expressed by the equation $p = bT - a$, where T is the absolute temperature, and b and a are constants depending on the substance and on its volume. An equation equivalent to this was found by Amagat to be applicable to gases; we find that it holds good for both liquids and gases, at any rate within the limits we have been able to reach in our experiments. In order to construct an equation which shall represent the results when volume, as well as pressure and temperature, is variable, it is necessary to discover the relation of the constants a and b to the volume, but this relation appears to be a very complex one.

On Wine Drinking, and its Effects on the Human Body.

By G. MUNRO SMITH, M.R.C.S., L.R.C.P.

CONSIDERING the growing importance of the subject of alcoholic drinks, and the enormous consumption of wine in this and other countries, I thought it might be of interest to gather together what is known as to its action on our tissues, endeavouring to sift out what is merely conjectural or false from what is reliable. With this object I have put together the following notes. Of the other vehicles by which alcohol is taken, I wish to say nothing, the subject is too large; and I am inclined moreover to think that amongst the most important part of the community, the educated classes, the effects, good or bad, of alcohol are generally due to wine, not to spirits or beer. Neither do I wish to touch upon the questions of morality, social or individual, which the subject naturally evokes.

There are certain corollaries which inevitably follow any definite knowledge as to the physical effects; but until that knowledge is tolerably certain, such deductions can only lead to confused assertion and denial, not to rules fit for any man's guidance.

We must distinguish at the outset between wine-drinking in large and in small quantities, between constant and occasional use, and between strong and weak wines. Now-a-days wine is usually drunk in much smaller quantities than formerly; our reputation for deep potations is not so great as it used to be, in spite of the amount sold. "I learned it" (my song), "in England, where, indeed, they are most potent in potting," says Iago: "your Dane, your German, and your swag-bellied Hollander are nothing to your English." This is still true, but not so true as formerly. The "falling off" is partly, I think, due to the alteration in the human constitution bequeathed to us by our intemperate ancestors, and partly because drinking is not so fashionable. Men are now often ashamed to take too much. In reading the biographies and histories of the last century one is struck by the fact that drunkenness was not looked upon as particularly disgraceful.*

Politicians, soldiers, and literary men were in the habit of frequenting taverns and drinking wine to an extent we can now only wonder at. Addison, Steele, Pitt, Theodore Hook, and many other heroes did not think it beneath their dignity to darken their brains with drink. The intemperance, both in eating and drinking, of our pious forefathers, can be read of in the pages of Thackeray and in the narratives of Lord Cockburn. Their habits are chiefly of interest here as showing what we have inherited, in many cases, from them.

Before noticing its effects on the organs and tissues of our bodies, it is necessary to say a word or two about wine itself.

The minute vegetable growth that produces alcoholic fermentation consists of innumerable oval cells capable of enormously rapid multiplication under favourable circumstances, such as moisture, nourishment, and temperature. M. Pasteur, who has studied the varieties of the genus Torula to which this microscopic plant belongs, came to the conclusion in 1883 that the quality of the wine depends as much on the kind of germ as on the soil, sunshine, and rain. There are probably several varieties concerned, such as the Saccharomyces ellipsoïdeus, Sacch. exiguus, etc. These minute organisms, floating in the air in summer, alight on the downy surface of the growing grape, and at the vintage are thrown into the vats with the fruit, where they work their changes on the grape sugar. Some clinging to unpicked fruit fall on the ground and perish, others are eaten by men and animals with the fruit to which they adhere, and passing through the body spend their winter, as Trouessart says, probably, in drain refuse, until the summer.*

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* De Quincey ("Confessions of an Opium-Eater") mentions that a certain duke used to say, "Next Friday, by the blessing of Heaven, I propose to be drunk."

The process of fermentation soon begins when the grapes are thrown into the vats. Pressure is usually applied to squeeze out the juice, but in some Rhine wines the weight of the mass of grapes is all that is used, and these are considered the best. The methods of preparing have a direct bearing on the subject, because there is an immense difference, not only in flavour, but in the actual dietetic value between good and bad wines; so much so, that they can hardly be classified under the same heading, either as beverages or medicines. In order that the ferment may act efficiently on the solution of grape sugar, albuminous matter must be present to forward its growth; also certain salts. These exist in the grape in the form of small quantities of vegetable albumen, with salts of sodium, potassium, and lime; especially tartrates. If all the sugar present is changed by the Torula, and the carbonic acid gas permitted to escape, we have a "still" wine, such as claret and hock; moreover, if no sugar is left, it is said to be "dry." If albuminous matter is deficient, the ferment has not sufficient vitality to change all the sugar, and a "sweet" wine results. If this is due to excess of grape sugar, there may be more alcohol present than in the "dry" wines. If the still incompletely fermented fluid is bottled, the imprisoned carbonic acid gives rise to effervescing or "sparkling" wines.

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* "Microbes, Ferments, and Moulds." By E. L. Trouessart.

Whilst in the vats, but still more afterwards by proper keeping, a characteristic taste and odour, the "bouquet" develops. This is partly due to the formation of ethers, especially œnanthic, in small quantities. The vegetable acids and acid salts of the fruit diminish as fermentation goes on. The colouring matter comes from the skin of the grape. In most wines there is a variable amount of tannin.

We have therefore rather a complex fluid to deal with; for we have salts, œnanthic and other ethers, a small amount of albuminous matter (in dry wines especially), occasionally vegetable acids, often a considerable amount of grape sugar, and usually a small, sometimes a moderate, amount of tannin. The action of wine on the tissues is the sum-total of the action of these ingredients. We may dispose of the *albuminous* matter very shortly; there is not enough in any kind of wine to have any effect. No nourishment can be expected from this source.

The *salts* are much the same as those found in the fluids of our bodies; they slightly stimulate certain secretions, otherwise they are inert.

Tannin, when in considerable quantity (which is rare in ordinary wines), slightly retards digestion in the stomach. It is found chiefly in coloured wines, but I have tested a sample of sherry with a fair quantity in it. In a pamphlet on the subject, I find it stated that "natural wines rich in tannin are the most wholesome." This, I believe, is quite erroneous; like strong tea, they retard the digestion of albuminous foods. In health I cannot see how they can be wholesome; in certain dyspeptic conditions associated with copious secretion of gastric juice, or catarrhal states of the stomach, they might do good.

The ethers in wine, although small in amount, are important, because they give the distinctive taste (together with traces of essential oils), confer some stimulating properties, and perhaps by their easy diffusibility help to cause the rapid action of wine on the circulation. The "bouquet" is therefore of value. It is absent, or nearly so, in certain common kinds of wine.

The carbonic acid has an action of its own of a stimulating character on the walls of the stomach.

Alcohol exists in the proportion of about 10 to 12 per cent., being lowest in claret and hocks, and highest in some kinds of port. In dry sherry there is about 15 per cent.; in champagne, about 15 per cent.; in hocks, 7 to 12 per cent.; and in St. Raphael wine, 15 to 16 per cent.

A moderate wine-drinker of to-day takes an ounce, more or less, of alcohol, diluted, per diem; "five-" and "six-bottle" men are dying out, and three or four glasses is considered a fair allowance with dinner.

In most people good wine begins to act directly it enters the mouth. This fact, long known to athletes, has been explained as follows: The interior of the mouth (palate, tongue, etc.), like all the rest of the face, is supplied by the fifth nerve. Stimulation of this nerve is said to cause dilatation of the vessels of the brain. Wine is such a stimulant; hence a freer flow of blood through the brain, and the feeling of increased activity.*

The effects of wine on the stomach (which depend in a very great measure on the state of dilution of the alcohol, the amount of tannin, acidity, etc.) begin quickly and do not continue long, absorption soon taking place. On albuminous food the action is unfavourable, except in dilute and small quantities, inasmuch as there is a certain amount of hardening. For example, chopped egg was mixed with a digestive fluid and placed in a uniform temperature of 99° F. in a glass heater. In another vessel an equal quantity of egg and digestive fluid had some claret added, another port, and another sherry. Mr. Baskett very kindly estimated the amount of egg digested in each case and found as follows:—

1.	Without any wine ...	... 2	centigram.
2.	With claret ...	... 1	,,
3.	,, sherry ...	... ·75	,,
4.	,, port ...	... ·65	,,

In the living body however we have various other factors to consider; on albuminous food wine retards digestion; it retards also the action of the gastric juice if above 10 per cent. alcohol is present; it seriously interferes when 20 per cent. is present: but it also does three other things; *viz.* it may increase appetite, it increases the vascularity and churning movements of the stomach, and it increases the secretion of the gastric juice.

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* See Lauder Brunton's "Materia Medica," where it is pointed out that all nations have the habit of stimulating this nerve when in deep thought; *e.g.*, taking snuff, scratching the head, rubbing the nose or chin, etc.

Dr. Gluzinski* came to the following conclusions:

1. That alcohol (as in wine and *dilute* spirits) disappears rapidly from the stomach.

2. That there are two phases of its action: (*a*) a marked decrease in the power of digesting albuminous bodies; but (*b*) after this, when the alcohol has been absorbed, digestion is more rapid than normal, and there is a more abundant production of hydrochloric acid.

3. That these two phases do not seem to occur regularly in diseased conditions, and in most forms of dyspepsia wine is bad.

4. That a small quantity of dilute alcohol (*e.g.* wine) exerts a favourable influence on digestion in healthy persons.

In the main I consider the above correct, but should slightly modify Nos. 3 and 4, thus: In health, a small quantity of good wine does no harm; in dyspepsia, with congestion or free secretion of gastric juice, it is probably bad; in dyspepsia, with anæmia and insufficient movement of the stomach and small secretion of gastric juice (as in many people leading sedentary lives), sometimes good.†

When absorbed into the blood, the ethers of wine seem to be rapidly eliminated; the alcohol forms with hæmoglobin a new compound which takes up and gives off oxygen less easily; hence wine in moderately large doses (1) prevents oxidation and waste of the tissues, and (2) lowers the temperature of the body by dilating the vessels of the skin and causing an increased flow of warm blood on the exposed surfaces.

Wine—and indeed alcohol in all forms—is almost unanimously avoided by arctic travellers, and it is interesting to note that in cold countries, such as Canada, Finland, and Norway, the amount consumed is very small compared with warmer climates.* In large doses the diminution of temperature from lessened oxidation is very marked.

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* "Deut. Archiv. klin. Med.," vol. xxxix. 1886.

† It is impossible to lay down infallible general rules on this subject. Statements are constantly made in an *ex cathedrâ* manner, but must, like the above, be received with caution.

The question, "Is Wine a Food?" may be answered by the preceding statements. It undoubtedly prevents waste; very little leaves the body as alcohol; this means that it undergoes rapid change, and is therefore a source of energy. Consequently it can take the place of food to some extent *in the absence of the latter.* Hammond, on insufficient diet, actually gained in weight when he took wine. In virtue of any directly nourishing properties, wine can never, by any possibility, become a food.

On the tissues generally the effects of wine seem to depend on the alcohol it contains, and on that only. In excess it leads to increase of connective tissue chiefly by direct irritation, partly by diminishing oxidation.

In investigating its action on the nervous system, we must distinguish (1) its effects on the cranial circulation, (2) on the nerves themselves.

1. By dilating the arteries of the brain it increases the activity of the nerve centres for a time, especially the rapidity of thought. Sheridan, Hartley Coleridge, Addison, and many other wits are said to have felt this. Theodore Hook could not give his wonderful improvisations, which were "wont to set the table in a roar," either very early or very late in the evening; before his faculties were warmed by wine and conversation he was unsuccessful. Later on, alas! he was, I suppose, apt to be uncertain in his utterance.

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* France heads the list of wine drinking countries with 119·20 litres per head per annum; in Canada, the annual consumption of alcohol in all forms is 3·28 litres per head.

This result of quickening and brightening conversation has been attributed to the dulling the perception of observation; *i.e.* the relief of nervousness. Dr. Johnson says: "Before dinner men meet with great inequality of understanding; and those who are conscious of their inferiority have the modesty not to talk. When they have drunk every man feels himself happy, and loses that modesty, and grows impudent and vociferous; but he is not improved, he is only not sensitive of his defects." "Wine only animates a man; it puts in motion what has been locked up in frost, but this may be good or bad." *

Johnson however was singularly free from what is called nervousness, and for other reasons was not an unbiassed judge. There seems to me plenty of evidence that wine, probably by its action on the cranial circulation, increases that form of wit which consists in the association of objects not usually associated and in detecting unexpected resemblances.

Many of the qualities attributed to wine drinking, such as increased generosity, are due to the pleasant sensations it often engenders, and to the frequent surroundings of company and food, the body being satisfied and contented.†

On the nerves themselves the action of wine, as of alcohol in other forms, is almost entirely in the direction of diminishing function. The nervous system appears to be affected somewhat in the following order: 1, Judgment; 2, inhibition (control); 3, motor centres; 4, cerebellum; 5, muscles generally; 6, respiratory centres; 7, heart. Under No. 3 comes the interference with the power of speech; and under No. 5 I should like to point out that the characteristic half-drunken expression of face with the eyebrow raised and the eyelid half shut is due to the early paralysis of the Levator Palpebræ, and the consequent increased action of the Occipito-frontalis to keep the eyes open.

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* See Boswell's "Life of Johnson." Boswell after plaguing Johnson on this question at length, said, "You know, sir, wine makes us forget what is disagreeable; would not you allow a man to drink for that reason?" Johnson: "Yes, sir; if he sat next you."

† "And body gets its sop and holds its noise,
And leaves soul free a little."
"Bishop Blongram's Apology." By R. Browning.

Of the excessive use of alcohol I do not wish to speak, except to say that it acts as a very sure, if slow poison; and that its action on the tissues, and especially on the nervous system of the parent, is very commonly handed down to the offspring, often in some form of mental aberration.

Summary and Conclusion.

We may, I think, say in conclusion, that wine in its effects on the human body must be looked upon as a mixture of several ingredients, and its action is therefore complex.

That in moderate doses in healthy people, provided it is not too strong, it rather aids than retards digestion, but in my opinion, in the *perfectly* healthy, it does neither; in strong doses it certainly very materially retards it. In some conditions of ill health it may be beneficial, but no broad rules can be laid down. The feeling that wine is wanted is certainly not an indication that it would be of use. On all points of the body its action depends on the amount taken and the degree of dilution; weak wines are almost always preferable to strong, unless an immediate effect is wanted.

I must again repeat that I believe good wine is so different in its action from bad, that they can hardly be considered to belong to the same class of drinks.

The Crossing of Ferns.

By COL. ARTHUR M. JONES.

BEFORE entering on the subject of this evening's paper, I should like, with your indulgence, to say a few words in justice to a naturalist of no mean order, who, though perhaps scarcely known to any of you, lived, and for many years did good work, in your own district—I allude to the Rev. Charles Padley, formerly of Bulwell Hall, Nottinghamshire, and who died last year Rector of Enville.

Mr. Padley was a born naturalist, and though chiefly distinguished as a botanist, his interest was not confined to that branch of science. To him, more than any one else, are the lovers and cultivators of British ferns indebted, for he was one of the most original, intelligent, persevering, and successful of discoverers, and one of the most generous also; and as, in addition to other obligations which I, in common with others, owe him in connection with such matters, I have been chiefly indebted to him for the material with which I have worked in my experiments in "The Crossing of Ferns," I feel it only just to make some acknowledgment.

When Mr. Padley, leaving Nottinghamshire, made his home in the West of England, and began to interest himself with British ferns, it was not long before he was irresistibly attracted by the exceptional beauty and almost exhaustless power of deviation of Polystichum angulare—"*The* Fern of the South." And it was not unnatural that after a time, appreciating it as he did, he should be desirous that some record of the more beautiful and marked varieties of this fern should be left; and that possessing himself considerable artistic skill, great natural discrimination, and intense interest in the subject, he should have contemplated producing "a monograph of Polystichum angulare." Having at that time in his employment as a gardener a young man of great intelligence and general fitness for the work, he sent him to London to be instructed in the best system of nature printing. It was not long before young Mr. Thos. Smith had mastered all the details of the art, and, being of an inventive turn, had hit upon several improvements in the process, which enabled him to produce an effect superior to any previously attained in this direction.

Unfortunately, however, before Mr. Padley had done more than collect material for his work, circumstances occurred which forced him to abandon his design, to the lasting regret of all interested in such subjects; and probably the valuable results of Mr. Smith's training would have been lost had I not by accident become aware of his talent, and been instrumental in employing him, in the interest of "The British Pteridological Society," to produce some nature prints of the varieties of British ferns. Subsequently I employed him myself in the issue of a more comprehensive series of nature prints in illustration chiefly of the more marked characters which run more or less completely through the different species of British ferns. This work, extending to between 200 and 300 plates, was concluded a few years ago; but since that time, many interesting experiments have been made in "The Crossing of Ferns," and it occurred to me that some record on this point also might add to the interest and completeness of the work, and that, as very many of the experiments referred to had been made in this neighbourhood, the subject might not be without interest to your Society, who I thought were thus clearly entitled to the offer of the earliest information. Hence my proposal to read a paper. It had always been my intention that one copy of "The Nature Prints" should remain in Bristol; and as your ever-watchful Secretary has told me that the natural place for that copy is in the library of your Society, I will only add, that if, when completed, it shall appear worth your acceptance, you are very welcome to it.

The crossing of ferns, like other new truths, has had to go through all the different stages of ridicule and incredulity, until the convictions of a few have at last forced conviction upon the majority, and the fact has received public recognition. I claim for the much-despised race of British fernists the credit of having established the truth in this instance.

Until comparatively recent times it was generally accepted that ferns did not cross; and yet, considering the assistance which, in their endless changes of structure, other forms of life were known to derive from the power inherent in them of crossing naturally, it must at times have seemed strange to the more thoughtful that in a class of plants so remarkable for variation as ferns that power should be altogether absent.

No doubt the faculty of natural variation (independently of crossing) is great in ferns, as elsewhere; and no doubt, also, in certain species of ferns, and of British ferns especially, this power of natural variation is so conspicuous that the more cautious botanists hesitated for a time—and perhaps for too long a time—to give attention to the conclusions of those who had carefully studied the subject, and thus the acceptance of the truth was retarded. It was not until a foreign fern or two had done what many British ferns had long been known to have done, that the serious attention of botanists was drawn to this subject; and I believe I am correct in saying that the first authoritative recognition of the fact was contained in a letter to Mr. E. J. Lowe from Sir Joseph Hooker about the year 1884, in which the latter used these words:—"The hybridization of ferns is now an accepted fact." But in the general ignorance formerly prevailing with regard to the reproduction of cryptogamic plants, it seemed altogether preposterous that ferns could possibly cross. No bee or fly had ever been suspected of visiting a fern or moss with any such intention or result, and enough was known of the structure of a fern to preclude the idea of any external agency in fertilization; enough was known of the history of a fern from its first leaf until the completion of what appeared to be its final act, viz., the scattering of its spores, to negative the idea that the spore was the result of sexual action; it was known not to be a seed, but beyond that, I believe, nothing was generally known than that by some mysterious transformation, apparently not altogether unlike the threefold changes in insect life, the fern produced the spore, the spore developed into the prothallus, and from the prothallus came the fern.

The Reproductive Organs of Ferns.

It was not until Naegeli, Suminski, Hoffmeister, and other continental botanists had raised the veil which had so long hung over the secrets of the reproduction of ferns and other kindred forms, that people were able to recognise that, however different in the process, the principle involved in the reproduction of ferns was the same as in higher forms of life.* It was they who showed that it was in the prothallus stage that the fern practically flowered—that on the prothallus were developed the male and female organs (the antheridia and archegonia) corresponding with the stamens and pistils of flowers, and that under certain favourable conjunctions of heat and moisture the antherozoids (corresponding with the pollen of flowers) were detached, and not only so, but were endowed with motion resembling rather the consciousness of animal than the impassiveness of vegetable life, and that thus fertilization was ensured (one archegonium only, I believe, being fertilized in each case, and one fern only produced from any prothallus); and as each prothallus contained all that was necessary for the reproduction of the plant, it was not suspected that antherozoids could have power of action beyond the sphere of their own prothallus, and it is sufficiently evident that, unless they had that power, ferns could not cross.

I have not a word to say against antherozoids generally—they are, I believe, as a rule, a very steady, stay-at-home race of little zoids; but there are exceptions—occasionally there are some that show a vagrant tendency—hence the results that we have to chronicle.

Concurrently with, but independently of, the researches of Hoffmeister and others—independently also of each other

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* Since writing this, I have been favoured by Dr. Masters with the following note:—

"Antheridia and antherozoids were discovered by Naegeli in 1844; the archegonia by Suminski in 1846, who also witnessed the entry of the spermatozoids into the archegonium. Bernhardi, according to a statement of Regel in the *Botanische Zeitung*, 1843, was the first to announce the production of hybrid ferns. Regel's observations are noted by the Rev. M. J. Berkeley, in the *Gardeners' Chronicle* for July 27, 1844, where also the remarks of the late Mr. Henderson on the subject are given."

—there were, among the earliest cultivators and students of the varieties of British ferns, some who were occasionally struck by the appearance of forms bearing a very suspicious resemblance to combinations of other forms; and attention having been arrested, observation became keener: and as in course of time these instances became more numerous, cultivators could not at last resist the evidence of their senses, that, in some way, unknown to them, ferns crossed; and experiments, undertaken with the view of more completely testing the correctness of their impressions, placed the matter entirely beyond doubt in the minds of all who had given attention to the subject.

PIONEERS.

When independent conclusions have been formed, nearly about the same time, by several, it would be unjust to transfer to any one in particular all the merit of a discovery. There were, I believe, in this case five who had formed more or less certain conclusions on this subject. I will therefore mention their names alphabetically—and fortunately this will bring to the front one to whom I am sure no true fernist will grudge any distinction; for no one has worked harder, and with less assistance, and to no one are lovers of ferns and mosses more indebted than to Mr. J. M. Barnes, of Milnthorpe, who, by his discoveries in Lastrea montana, has opened up an entirely new field of interest, though I do not mean to imply that this is the only claim which Mr. Barnes has on the gratitude of all who are interested in these matters.

Next in alphabetical order is Mr. E. J. Lowe, the well-known author of *Our Native Ferns*, and distinguished in so many other branches of science that no further remark is needed here.

Next, Mr. J. E. Mapplebeck, of Hartfield House, near Birmingham, than whom a more observant, careful, and successful cultivator of British ferns, or a more uniformly distinguished exhibitor of them, was never known.

Next, Mr. James Moly, one of the most original, indefatigable, and successful of discoverers, formerly of Hawkchurch, now of Charmouth, who, with one or two others, has done in the South with Polystichum angulare what Mr. Barnes and others have done with the Lastreas in the North.

Last, but not least, Mr. Stansfield, the elder, a botanist of no mean order, and the original very enterprising head of the well-known firm at Todmorden—now chiefly represented by his grandsons, Messrs. Stansfield of the Sale Nurseries, near Manchester.

To these five, I believe, is due the credit of having been the first to recognise practically the fact of the crossing of ferns. There are, however, others who, though coming later into the field, have contributed so materially to its general recognition that this statement would be incomplete without alluding to them.

Of these the first entitled to be mentioned is the late Mr. A. Clapham, of Scarborough, so long known as one of the most observant, painstaking, and generous of the early discoverers and propagators of the varieties of British ferns. Mr. Clapham was long known as the most successful of all hybridizers of plants in England, but through want of faith in this instance he long held aloof from experiments with ferns; but when at last (impressed by some of the results of Mr. Lowe's experiments) he did give attention to it, he approached it with all his old keenness of perception and judgment, and therefore his old success.

The writer of these notes is perhaps entitled to come next; and it is a pleasure to him to think that his experiments were not without influence on two others, who, in conjunction with him, have helped to produce an amount of evidence on this subject so overwhelming as to be absolutely convincing to all not predetermined to remain unconvinced. He refers to Mr. E. F. Fox, of Brislington, near Bristol, one of the most painstaking and successful cultivators and raisers of British ferns; and the late Mr. W. C. Carbonell, of Rhiew Castel, Usk, who was not less distinguished in both these respects, and who has lately with great public spirit bequeathed the whole of his fine collection of British ferns to Kew Gardens.

HYBRIDIZATION.

A year or two since, I was told by one of the most discriminating botanists, Mr. Churchill, that it was now accepted that Asplenium germanicum was a hybrid between A. septentrionale and A. ruta-muraria; and as Mr. G. B. Wollaston, of Chiselhurst, *facile princeps* among British fernists, is entirely of the same opinion, the idea cannot be lightly regarded. I am told that A. germanicum is never found where A. septentrionale and A. ruta-muraria are not found, and that where these two species abound it is very rare not to find A. germanicum—that it is never found extending over a considerable space as ordinary species do, more or less, but in detached clumps or isolated plants as hybrids, having a difficulty in reproducing themselves, might be expected to do. It is reputed to be barren, and yet it is on record that Sim, of Foots Cray, once raised a variety from it, recorded as "acutidentatum" both by Mr. Moore and Mr. Wollaston; it cannot therefore be said that it is impossible that a spore might not be thrown from one of such hybrid plants with sufficient constitution and general fitness for survival to establish a new race. There is also the case of Lastrea remota, of which four plants were found growing in a clump in Westmoreland, by Mr. F. Clowes, about thirty years ago; nor has it been found elsewhere in this country; it has long been accepted by British fernists as a natural hybrid; though apparently profusely sporiferous, it has, after what might have been considered exhaustive experiments, been judged incapable of reproducing itself from spores, and yet it is, I believe, sufficiently established that one of the original plants which Mr. Stansfield has, did once cover the pot in which it was with fertile spores; and though the young plants may not yet be in every respect entirely like the parent, they are, I believe, sufficiently unlike every other British fern to prove their origin: and would it not be unreasonable to conclude that not one of these could have power to reproduce itself with ordinary freedom?

The remarkable "confluent" forms of Asplenium Trichomanes, as fertile in appearance as they have hitherto shown themselves to be barren in reality, are considered by those who have most studied the subject to be hybrids, as are also the "microdon" forms of A. Adiantum nigrum and A. lanceolatum. There is also the unique cruciate Asplenium, which appeared self-sown in Mr. Clapham's fernery, and the remarkable hybrid Asplenium found by Mr. Wollaston in Switzerland. There are two or three other forms marked apparently with the bar sinister, but I pass to less doubtful cases.

To Mr. E. J. Lowe is due the credit of having been the first to raise an unmistakable hybrid between two acknowledged species, which was neither a monster of ugliness nor incapable of reproducing itself from spores. It was no accident, for with deliberate intent Mr. Lowe set himself to produce a cruciate Polystichum aculeatum by crossing a very narrow cruciate form of P. angulare, well known as "Wakeleyanum," with a very robust form of aculeatum, equally well known as "densum"—two more marked forms could not have been selected. In his first sowing he was rewarded with four plants as narrow and symmetrically cruciate as Wakeleyanum, but with all the coriaceous texture, the glistening colour and decurrent pinnules of P. ac. densum; and on repeating the same experiment he was rewarded with two other plants. A grander form does not exist among British ferns—the most convincing proof of which lies in the simple fact that even people quite ignorant of ferns hardly ever pass by the plant which Mr. Lowe kindly gave me, without remarking, not a little to my disappointment, that it is the most striking plant in my collection! The judgment passed by Mr. Lowe on this form is, that it is barren, and so the plant with which he has himself experimented may be; but I have equally good grounds for knowing that the plant he gave me is not so, for it has been proved by myself and others to be easy of reproduction from spores. If this distinction between two of these plants be real, it will be recognised as a most instructive fact; and I should also mention that a batch of seedlings, raised from another of Mr. Lowe's four plants by Mr. Carbonell, were, without exception, cruciate but premorse and dwarf. Nor is it without interest in connection with this matter to mention that Mr. E. F. Fox once raised from a magnificent plant of P. ac. densum an abundant crop of plants almost entirely premorse and dwarf. But, it may be said by some, "After all, this only proves that P. aculeatum and P. angulare are forms of the same species." If so, I will simply ask them to define clearly and fully what a species is.

Subsequently to this, two other instances of a clear cross between these two species have occurred. Mr. E. F. Fox, from a sowing of a very marked form of crested angulare* raised accidentally about a dozen plants of P. aculeatum, in which the peculiar cresting of the angulare in question was reproduced in an unmistakable manner—the crossing having evidently been effected by stray spores from an almost normal plant of P. aculeatum, growing in close proximity with the plant from which the spores had been gathered; and I myself raised two plants in which the polydactylous cresting of an angulare reappeared in a decided aculeatum—a polydactylous aculeatum having been previously unknown.

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* Cristatum, Wollaston, No. 10.

Nor should Mr. Stabler's remarkable Lastrea (so well known as L. f. mas Stableri) be passed over here; it has been universally regarded by fernists as a cross between two species of the male fern, the marked character of L. f. mas Barnesii being transferred to the more solid substance of L. ps. mas. It is easily reproduced from spores.

CROSS-BREEDING.

So far the crossing of species, or hybridization proper. I will now take a few of the most marked instances of the crossing of varieties, nor will I mention any with regard to which doubt could be likely to arise in any reasonable mind, and I will not confine the instances to any one species.

Years ago, returning from a successful hunt, Mr. Barnes thought he would put to the test the suspicions regarding the crossing of ferns, of which he could not rid his mind; he accordingly chose two of the most marked forms of Lastrea propinqua, both of his own finding—(1) a truncate form, whose claim to distinction lay in the entire deficiency of the upper part of the frond (to the extent of about a third); (2) a very symmetrical crested form, the best of its class—L. p. cristata of Barnes. Spores of these were sown together, and the result was, I should think, sufficiently conclusive—one plant being an exact reproduction of the truncate form with the addition of the cresting of the pinnæ; other plants resembled the crested parent, with depauperation so slight as not to affect the general symmetry of the frond; and there were intermediate forms between these. Another successful cross of Mr. Barnes was between two perfectly distinct forms of A. F.-fœmina, Frizelliæ and Craigii. Frizelliæ, as is well known, has a way of occasionally sending up a normal frond, and the crested forms of Frizelliæ produce also ordinary crested fronds; but the plant of Frizelliæ in question, whenever it attempts to revert, or rather to show its other parent, sends up an unmistakable frond of Craigii, a thing never seen before.

Mr. Moly's most marked instance of crossing was with a Scolopendrium. He had found a very remarkable variegated form, with bright green bands on a white ground, the latter presenting the appearance of being deficient in one of its tissues, the green portion standing out above the other portion of the frond. Mr. Moly was not quite satisfied with this fern, thinking it too narrow, so he sowed it with a broad form, "fissum latum" of Moly, with a view to a cross; and he got a plant of fissum latum variegated in the unique manner above referred to.

An experiment by Mr. Mapplebeck with Scolopendrium was not less remarkable. He sowed a conglomerate form with a form of peraferens with astonishing results. In several of the plants the conglomerate character is retained, but frequently little pouches are distinguishable at the extremities of the leafy portion. It is well known that the conglomerate forms have a way of occasionally throwing a normal frond, but in the case of these crosses fronds of simple peraferens are thrown, truncate with horn and pouch. Another very striking cross of Mr. Mapplebeck was with Pteris aquilina. I had myself raised a crested flexuose P.

aquilina, a cross between P. aq. cristata of Glover and the flexuous form (glomerata of Jacob Jones). Mr. Mapplebeck was so much struck with this plant that he determined to try the same experiment, using spores from his own garden. He raised at least three plants of the same cross—one much more marked than my own.

Mr. Stansfield's first experiment was with the cruciate form of Athyrium, Pritchardii. Thinking that a crest at the end of the frond would be an improvement to it, he perseveringly sowed crested forms with it until he succeeded in his wish; he tried the same experiment with the congested form, Grantiæ, with similar success. The cresting of ferns is, of course, no absolute proof of a cross, as it is known to result in many cases from natural development; but the following case speaks for itself:—Mr. Stansfield sowed his own splendid plumose form of Athyrium with Craigii, and he produced an unmistakable plumose Craigii. It was barren, and papery to an extreme; and, as if to settle for ever all doubts as to its origin, it produced last year a frond of the typical plumosum, simply crested, without a taint of Craigii except the cresting. Mr. Stansfield also raised a remarkable cross between A. F.-f. congestum and Craigii.

It is difficult to say whether Mr. Lowe's crossings of Scolopendrium or Athyrium were most marked. In the former, almost every conceivable combination of undulatum, multifidum, marginatum, and muricatum are to be seen in his garden; and it is the same with Athyrium. It was Mr. Lowe's extraordinary series of combinations between Victoriæ and proteum, etc., which roused Mr. Clapham to exertion.

Mr. Clapham confined his experiments to Polypodium vulgare, his most marked success being with the finely-cut

Davallia-like form known as Cornubiense; he sowed with this a multifid form of bifidum. Here, again, the proof is conclusive. Cornubiense is well known to have a habit of partial reversion—sometimes it is the whole frond, at other times it is a portion only that reverts; these crosses of Mr. Clapham (of which there were about six) revert to an ordinary multifid bifid frond, while the finely-cut portion is forked at the apex of the frond and the apices of the pinnæ, exactly as was to have been expected in such a cross.

I come now to my own experiments, and I must admit that they were originally the result of accident, and that I am mainly indebted to the late Rev. C. Padley for the materials with which I worked. I have before alluded to the extent to which fernists generally are under obligation to Mr. Padley. The late Dr. Wills was another of this class; and if one mentions in connection with these and with those before alluded to, the names of Dr. Allchin, Mr. Grey of Exeter, Mr. Jackson of Barnstaple, Mr. James of Vauvert, Mr. Clowes of Windermere, Mr. Glover of Manchester and Southport, and Mr. Phillips of Belfast, one will have mentioned all the original pioneers to whom the present and future generations of British fernists are most indebted.

Mr. Padley had given me, among others of his best ferns, three very marked forms of P. angulare, his polydactylum from the Vale of Avoca, his multilobum ovale (the divisions of whose pinnules are not less marked than pretty), and his inæquale variegatum, an entirely unique form. Being very much struck with the superiority of Mr. Padley's polydactylum over all other crested forms of angulare, I sowed it often freely with other forms, but keeping a record. Without expecting a cross I sowed it with the cruciate form of angulare before mentioned (Wakeleyanum), and I got two plants of a polydactylous cruciatum. I sowed it with multilobum ovale, and got four plants of polydactylum with pinnules divided in exactly the same way as multilobum ovale; and I sowed it with inæquale variegatum—the result being five plants of a character so marked as to be conclusive. Convinced at last of the reality of crossing, I sowed in faith; and as Mr. Fox and Mr. Carbonell followed on the same lines, there is now hardly a distinct form of angulare that is not polydactylous; there are now polydactylous divisilobes, there are frondose and decomposite forms; forms also of lineare, congestum, and flexuosum all polydactylous. Mr. Fox's most marked success was, perhaps, his polydactylous congestum, of which he raised many plants; but his cross of Athyrium F.-f. reflexum with Craigii is not less marked. Mr. Carbonell's crosses between P. ang. divisilobum of Padley, and grandiceps, Moly, and crosses between polydactylum of Padley and other forms, are well worthy of note.

It is, I think, now perfectly clear that it only requires patience and ordinary skill to effect endless crosses in almost every species, for there is now hardly a species of British fern in which there are not sufficient varieties to begin with, and there are several species in which an addition to the existing varieties would well repay any exertion—for instance, in Osmunda regalis, Pteris aquilina, and Lastrea recurva. On the other hand, care should be taken to discriminate between what are really new varieties and mere trifling differences, and this partly from a sense of justice to others, and partly to prevent flooding collections with interminable repetitions. The practice of raising from well-known forms and putting in a claim for the general result, is as little to be defended on grounds of logic as of morality. Surely there should be a generous rivalry between all engaged in such a pursuit to accord to others what is their due.

How to Effect a Cross.

It only remains to give a few hints how to ensure the greatest chance of success in crossing. It is clear that the closer the juxtaposition of the prothalli the greater the chance of the antherozoids from one prothallus straying on to another, therefore thick sowing is a consideration; but as thick sowing is a fruitful cause of an unhealthy state of the prothalli (which is generally the destruction of the whole), extreme care should be taken to ensure healthy conditions in every respect. And first, perhaps, in importance is it, that spores *only* should be sown: for this purpose it is best to have an abundance of spores, and so to manipulate them as to sift them well; with care, and the skill which comes easily with a little practice, this can be done so that practically only spores remain. But even more important than that is it that the spores which it is intended to cross with should have come to maturity about the same time; for it is well known that spores that have been gathered some time do not germinate so soon as those that have recently dropped, and of course, unless the prothalli are fit for fertilization at the same time, the whole chance of crossing is lost.

Other probable aids to crossing may be a judicious watering overhead just at the right time, and possibly it may be a further help either to sow on an incline, or afterwards to tilt the seed-pan. I have only, in conclusion, to add, that where so much is necessarily dependent on chance, one must not be too sanguine; with care, however, and perseverance, results are certain to follow in due course.

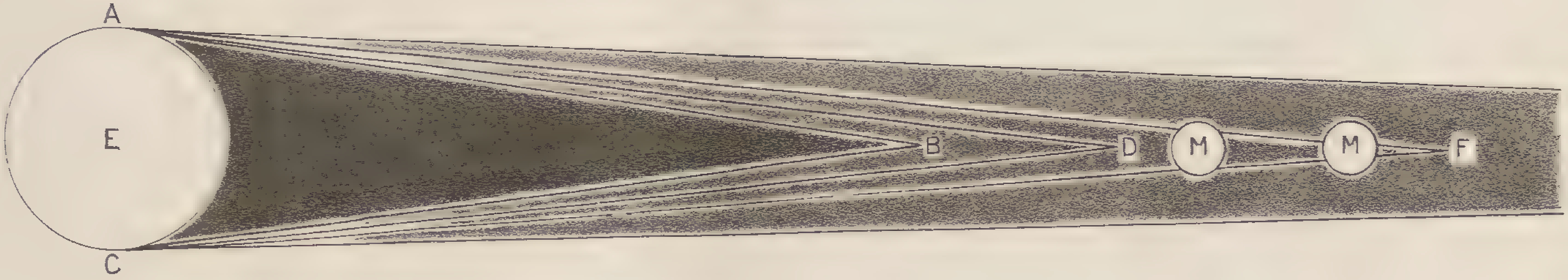


Diagram to illustrate Illumination of Eclipsed Moon.

The Illumination of the Eclipsed Moon.

By GEORGE F. BURDER, M.D., F.R.Met.Soc.

Read May 3rd, 1888.

IT is a matter of common knowledge that the moon, in a total eclipse, does not, as a rule, entirely disappear, but remains as a tolerably conspicuous object in the heavens, shining with a dull red light throughout the total phase. The amount of light varies. In some eclipses, the moon has been so bright that people have been led to doubt whether the astronomers had not made a mistake. In other eclipses, the moon has been so dull as to be practically invisible. Eclipses are recorded in which the moon was said to be absolutely invisible, the sky presumably being clear; and in October, 1884, we had ourselves the rare opportunity of observing an eclipse in which the moon, although not strictly invisible, was so obscure that many persons in looking for it failed to discover it.

It has been suggested that these differences might be explained by differences in the clearness of the atmosphere at the time and place of observation, or by differences in the moon's altitude in different eclipses. Such circumstances cannot be without influence, and should be borne in mind when comparisons are made; but no one who watched, for example, the two last total eclipses visible in this country, could doubt for a moment that there are very great differences in the light of the eclipsed moon, apart from differences in the state of the atmosphere, or in the moon's position in the sky.

In a note on the eclipse of October, 1884, which is printed in our Proceedings, I stated what seemed to me very strong objections to the commonly received theory regarding the variation of the light in different eclipses, and I ventured on a suggestion that the solar corona might have something to do with it. The eclipse of last January led me to restate my views, and a correspondence ensued, which stimulated me to a more careful examination of the question than I had previously made. In a matter which like this has engaged the attention of eminent astronomers, there seems small chance of discovering anything that is both new and true; and yet I hope to be able to show that some important features of the question have been hitherto overlooked.

At first sight there seems no reason why the moon, when immersed in the shadow of the earth, should receive or exhibit any light at all, and hence an attempt to account for the variation in the amount of light (which is the special object of this paper) involves of necessity an inquiry into the possible sources whence the light may be derived.

Kepler, some three centuries ago, was the first to suggest an explanation of the phenomenon, and it is substantially his theory which is accepted by astronomers to-day. It is believed, first, that the illumination of the eclipsed moon is brought about by the refraction of the solar rays through the lower, and especially the lowest, strata of the earth's atmosphere; secondly, that in consequence of the amount of vapour in these low strata, the blue rays are largely absorbed, and the red rays chiefly transmitted; thirdly, that it is these red rays that give to the eclipsed moon its ruddy tint; and fourthly, that the degree of illumination of the eclipsed moon is dependent on the meteorological conditions of the atmospheric ring through which the sun's rays pass to reach the moon. Up to this point there seems to be a general agreement, but as to the *particular* meteorological conditions which favour a light eclipse or a dark eclipse, there is a divergence of opinion, some holding that the dark eclipses occur when the atmosphere is unusually dry, others when it is unusually humid, while Sir John Herschel attributed the variation to the varying amount of cloud.

The first of the above propositions—that the illumination is due to the refraction of the sun's rays through the lower strata of the atmosphere—may be admitted with two qualifications. In the first place, I do not think that the whole of the light is derived from the sun's refracted rays, although the greater part may be; and in the second place, I believe that the higher regions of the atmosphere, up to a certain level, are quite as much concerned as the lower regions. I shall have to refer to this point again.

That the ruddy tint of the eclipsed moon is due to the vapour in the air absorbing the blue rays and transmitting specially the red rays, is a view which I think open to question, and also unnecessary. Why should not the ruddy tint be the natural colour of the moon? It can scarcely be supposed that the moon has no colour. True we see none under ordinary circumstances, but then the colour may be extinguished by the excessive brilliancy of the reflected sunlight, only becoming perceptible when the illumination is much reduced, as in a total eclipse, or as in the crescent moon, when the shady portion of the disc, feebly illuminated by earth-light, assumes a tint not very unlike that of the eclipsed moon.

The views I have thus referred to are, however, chiefly of interest as leading up to the conclusion that the varying light of the moon in eclipse is to be explained by the varying condition of the atmospheric ring through which the sun's rays pass to reach the moon. The objections to this view, supported though it has been by the highest astronomical authorities, are to my mind insuperable. The particular modification of it which seems at present to be generally accepted, is especially open to criticism.

In an article in the *Times*, published on occasion of the eclipse of October, 1884, the writer explains the matter in these words :—

"If the part of the terrestrial atmosphere through which the solar rays pass be tolerably free from vapour, the red rays are almost entirely absorbed, leaving the blue rays, which give too feeble an illumination to render the moon visible; while, on the other hand, if the atmosphere be highly saturated, the blue rays are more effectually absorbed, and the red rays transmitted to the moon, thus rendering it visible."

According to this view, then, it is a saturated condition of the air which, transmitting specially the red rays, favours the illumination of the moon; while a dry atmosphere, absorbing the red rays, reduces illumination. But this, I submit, is exactly contrary to what we habitually observe. When the sun sets with a red disc, its light, instead of being increased, is so much reduced that we can gaze on it without discomfort. The same thing occurs when the sun is seen like a red ball through a fog. In certain states of the atmosphere the sun, even at setting, shows no redness, and then it is too bright to look at, even when the greater part of its orb has dipped beneath the horizon. Indeed I believe the law to be universal, that whatever state of the atmosphere reddens the sun also reduces its brightness.

Sir John Herschel's idea that the unusual obscuration of the moon in certain eclipses was due to an unusual prevalence of *cloud* in the ring of atmosphere through which the sun's rays would have to pass, is not open to the objection that it is at variance with observed facts, for we know that clouds will intercept the sun's rays. But it is open to an objection that applies to any theory which seeks to explain the variation in the moon's visibility by variations in the conditions of the earth's atmosphere. Consider the circumstances of the case. The atmospheric ring through which the sun's rays must pass is a ring 25,000 miles in circuit; and it is something more than a ring, for owing to the earth's rotation during the progress of the eclipse, a perpetual succession of rings will come into position. An eclipse of the moon may be total for an hour and a half or more, and in that interval a breadth of atmosphere will have been brought successively into the position of the ring, extending in the equatorial regions to about 1,500 miles. To me it seems in the highest degree improbable that a tract of atmosphere 25,000 miles in length, and many hundreds of miles in average breadth, embracing, moreover, every description of terrestrial climate, should vary materially in its aggregate or average condition at any two epochs.

The same line of argument may, I think, be carried a step further. It is important to note that, as far as our knowledge goes, the character of a lunar eclipse is persistent throughout its duration. The light eclipses are light from first to last; the dark eclipses are dark from first to last. Unless, therefore, we suppose a number of remarkable coincidences, we must believe that the particular condition which determines the lightness or the darkness of an eclipse is persistent through a length of time much exceeding the duration of the eclipse. If we suppose this condition to persist for twelve hours, and to be seated in the earth's atmosphere, it must be a condition involving nearly the whole of the atmosphere. To suppose that the atmosphere over, say, three-fourths of the globe can be greatly more humid or greatly more cloudy at one time than at another, would be manifestly unreasonable.

It may possibly be objected that the atmospheric ring presented by the earth to the sun and moon in a lunar eclipse may be very different in different eclipses as regards the relative proportions of land and water underlying it, and that its hygrometric condition, and therefore its transparency, may vary considerably from that cause.

I do not think this objection would weaken my argument materially, even were there no special reasons for rejecting the explanation which it offers. But there are two such reasons.

In the first place, it rests on the assumption that the lower strata of the atmosphere are those chiefly concerned in transmitting the rays which illumine the eclipsed moon, whereas I hope to show presently that that assumption very imperfectly represents the real facts of the case.

Secondly, the explanation will not bear the test of application to the phenomena of actual eclipses. If we compare the eclipses of March 19th, 1848, and October 4th, 1884, with respect to the proportion of land and water underlying the atmospheric ring, we find that in the middle of the former eclipse the proportion of land was very much less, and of water very much more, than in the middle of the latter eclipse. If, therefore, any effect at all is to be ascribed to this cause, the eclipse of March, 1848, should, on the more reasonable view of the effect of vapour, have been a comparatively dark eclipse, and the eclipse of October, 1884, should have been a comparatively bright eclipse. We know the facts to have been the exact reverse of this. Again, if we compare the eclipse of October, 1884, with the eclipse of January last, we find that the proportion of land to water at the time of mid-eclipse was almost identical in the two eclipses. Yet in the one case the moon was so dim as to be practically invisible; in the other it was at least of average brightness. These results were obtained by careful experiments with a terrestrial globe.

It has been supposed by some that the eclipse of October, 1884, may have owed its remarkable darkness to the presence in the earth's atmosphere of a vast quantity of volcanic dust from the Krakatoa eruption of the previous year, and that the other dark eclipses on record may admit of a similar explanation. I will only say of this theory that the cause seems to me inadequate to the effect, and that if the atmosphere generally had been charged with volcanic dust to the extent that the theory requires, more decided evidence of the presence of this material might have been expected than any we possess. In particular, we might have expected to find, after a sufficient lapse of time, copious deposits of dust over large tracts of the earth's surface.

The theory of the atmosphere being charged with cosmic or meteoric dust, or dust of extra-terrestrial origin, is, I think, equally open to the last-named objection.

I hope I may now claim to have shown that the problem under discussion has not, up to the present time, been satisfactorily solved, and that an effort to elucidate it further is not at all events superfluous.

In some remarks on the eclipse of October, 1884, which I made at one of our meetings, I threw out the suggestion that the eclipsed moon might possibly be in some slight degree self-luminous. Since the recent eclipse, I observe that the same idea has been put forward by an Italian astronomer. I think it is not altogether unworthy of consideration, for we know that there are substances in nature which even at ordinary temperatures are phosphorescent, and there are other substances which become phosphorescent when heated. The moon, having no atmosphere to shelter it from the fierce rays of the sun, must be exposed to intense heat, and might possibly continue to shine feebly after the solar rays had been withdrawn. Still, this explanation at the best is very incomplete, for it gives us no account of that which is the only real puzzle in the case, namely, the varying luminosity at different times.

The idea which I broached in my published account of the eclipse of October, 1884, deserves, I think, a little more attention. I suggested that the solar corona might be an important source of light to the moon during total eclipse, and that the varying amount of light might be connected with the known variations in the brightness of the corona at different times. The corona, as seen from the earth in a total eclipse of the sun, is a very bright object. It has been described as dazzling in its brightest part, and the total amount of light emitted by it has been estimated as equal to that of the full moon. The moon in eclipse will not receive nearly that amount of light, because the earth will intercept all the more central and brighter parts of the corona. Still, enough may remain to contribute a sensible quota towards the illumination of the moon, and in part to explain the variation in the degree of illumination. It has been suggested recently that this question might be settled by an appeal to the spectroscope. The eclipse of January last was examined spectroscopically by several observers. Some found lines in the spectrum agreeing with those found in the coronal spectrum. In other cases the results were negative or ambiguous. But I doubt if this question, from the nature of the case, can ever be settled by the spectroscope. We know from theory that light must reach the eclipsed moon both from the sun and from the corona, and it is not easy to see what more the spectroscope can tell us.

I know of but one method by which it might be possible to ascertain whether the solar corona has any appreciable share in the illumination of the eclipsed moon. It is the opinion of some astronomers that the brightness of the corona varies in cycles corresponding to those of the sun-spots, only that when the sun-spots are at their minimum, the corona is at its maximum. The sun-spot period is a little over eleven years, and if it were found in a long succession of eclipses (the relative brightness of which was carefully estimated or measured) that eclipses occurring about the period of sun-spot minimum were above the average brightness, there would be evidence in favour of the corona. But this, if ever done, must be a work of the future. The records of past eclipses are not accurate enough for the purpose, except in the rare cases in which either the brightness or the obscurity has been so abnormal as to attract notice. It would be rash to rely on these few instances. Yet it may not be out of place to remark that the two eclipses which represent the two extremes of modern times in regard to the degree of illumination both tend to favour the idea of the influence of the corona. The eclipse of March, 1848, which has often been cited as an abnormally bright eclipse, occurred (as nearly as I can ascertain) about two and a half years after a sun-spot minimum; and, on the other hand, the exceptionally dark eclipse of October, 1884, occurred about the time of a sun-spot maximum.

I pass now to a consideration of the matter from another point of view, and propose to inquire whether a new light may not be thrown upon it by a careful investigation of the course of the refracted solar rays through the different strata of the earth's atmosphere.

In the diagram annexed I have indicated the points at which rays from various parts of the sun's disc would, after refraction through the lowest stratum of the earth's atmosphere, converge upon the central axis of the shadow. The moon is also shown in its two extreme positions of perigee and apogee. The diagram is drawn to scale so far as regards the relative distances of these several points. The amount of refraction suffered by a ray which grazes the earth, traversing the entire thickness of the terrestrial atmosphere, is taken as one degree six minutes, or double the amount of what is known as "horizontal refraction." A ray from the *near limb* of the sun, having undergone that amount of refraction, would meet its fellow in the axis of the shadow at the point B, distant from the earth about 166,370 miles. A ray from the *centre* of the sun would strike the axis at D, distant 206,870 miles. And rays from the *further limb*, having undergone what, for distinction may be called cross-refraction, would converge at F, distant 273,450 miles. The point B would be the nearest point at which any refracted rays from the sun could reach the centre of the shadow, and the space within the angle

A B C would be, as regards solar light, absolutely dark. The point F would be the furthest point at which rays refracted through the lowest stratum of air would illuminate an object in the shadow.

And here I may remark that the rays which will reach any given point in the axis of the shadow after refraction through any given stratum of atmosphere, will be rays from an infinitely narrow ring of the sun's disc and from no other part. Confining our attention at present to the rays refracted through the lowest stratum, we note that the rays reaching the point B will be those only which have come from the extreme margin or limb of the sun. Points between B and D will receive rays from successively smaller and smaller circles of the sun's disc, until at D these circles will have dwindled down to the centre itself, and rays of the directly refracted class will have come to an end. But at the same point D, where the directly refracted rays cease, the cross-refracted rays begin to appear, and points between D and F will receive rays from successively wider and wider circles of the sun's disc, until at F we again meet with rays derived from the extreme margin of the sun. To put the case shortly: the earliest rays to converge will be rays from the *limb* of the sun refracted through *corresponding* parts of the earth's atmosphere; the latest to converge will be rays from the *limb* of the sun refracted through *opposite* parts of the earth's atmosphere; while rays from the *centre* of the sun will converge at an intermediate point.

We have hitherto confined our attention exclusively to the solar rays refracted through the lowest and densest stratum of the earth's atmosphere. But what of the rays which pass through the upper strata? Precisely the same series of events will happen to these, only each step will be lower down in the length of the shadow (or further from the earth) than those we have been considering. This conclusion will be perfectly obvious when it is remembered that the refractive power of the atmosphere diminishes as its rarity increases. If a line were drawn in the diagram representing the extreme range within which rays passing through the lowest stratum shed their light along the axis of the shadow, and similar lines were drawn for rays passing at heights in the atmosphere of half a mile, a mile, two miles, etc., these lines would overlap each other successively, the result being that an observer at any one spot (except just at the near end) would receive rays which had passed through an infinite number of atmospheric strata, derived from an infinite number of circles on the sun's disc.

We are now prepared to examine what the state of things will be at that spot in the shadow where the moon may happen to be placed. The two extreme positions of the moon are accurately shown in the diagram, and it will be seen how largely these positions differ in respect of their distance from the earth. The variation in the apparent size of the moon at different times must have been often observed by persons unacquainted with the cause. In truth, the moon is one-seventh part further off in apogee than in perigee, and its apparent diameter is therefore one-seventh part greater in perigee than in apogee. The moon eclipsed in either position will receive rays which have passed through the lowest stratum of the earth's atmosphere, although it will be beyond the spot where what I have called *direct* refraction through the lowest stratum ceases to take effect. Apart, however, from the rays received through the lowest stratum (which would probably give very little light), the moon in both positions will receive rays refracted through the higher regions of the atmosphere, *but* *with a difference;* and it is this difference that I wish particularly to insist upon.

I have calculated for each position the height in the atmosphere through which rays from the *centre* of the sun must have passed to reach the moon, and I have made a similar calculation for directly refracted rays from the *limb* of the sun.

In these calculations I have assumed that the mean barometric pressure at sea-level is thirty inches, that a solar ray passing through air of that density suffers a refraction of sixty-six minutes, and that a ray traversing the atmosphere at any given height above the earth suffers a refraction proportional to the maximum pressure which it encounters. In converting pressure into height I have followed tables constructed from actual observations. The following are the results:—

		Moon in Perigee.		Moon in Apogee.
Centre of sun	...	1,752 feet	...	5,761 feet.
Limb of sun	...	9,714 „	...	13,307 „

I cannot but think that we have here the principal cause of the differences observed in the visibility of the moon in different eclipses. I suppose no one will doubt that the transparency of the air increases with elevation, whether this be due to diminishing vapour or to increasing rarity, or to both combined. Moreover, it is certain that above the limit of the region of cloud there will be less obstruction to the passage of the sun's rays than below that limit. Now it so happens that the heights through which the moon in its two extreme positions will receive the sun's rays are just such that the difference of three or four thousand feet would be most important with reference to the amount of cloud. The inference, to my mind, is very clear, that the nearer the moon is to the earth in a lunar eclipse, the greater will be the obstruction in the way of the sun's rays reaching it; the farther the moon is from the earth, the purer and brighter will be the light which it receives.

In testing this view by a reference to actual instances, I would use the same caution that I used with regard to the corona. Yet I am tempted to mention that out of four eclipses, the particulars of which I find available, three lend a fair support to my theory. In the bright eclipse of March, 1848 (already referred to), the moon was within three days of apogee, and in the dark eclipse of October, 1884, the moon was within three days of perigee. The eclipse of January last, which was probably of about average brightness, occurred nearly midway between apogee and perigee. Where two or more causes combine to produce an effect, complete accordance with either one of them is not to be expected.

In all that has preceded, my remarks and calculations have had special reference to the central axis of the earth's shadow, or to the moon when centrally eclipsed. The problem of the moon's illumination, even when thus narrowed, is not free from difficulty; it acquires additional complexity when we include in our consideration the varying amount of illumination in different parts of the moon's disc, or of the whole disc in different stages of the eclipse—in other words, the varying amount of light in different parts of a transverse section of the earth's shadow.

Some of the differences observable in the brightness of different parts of the eclipsed moon—and possibly all the irregular or patchy differences observable—may be ascribed to differences in the light-reflecting quality of different parts of the lunar surface, these differences being revealed or magnified by the reduced illumination, just as the progress of the penumbra in a lunar eclipse may be rendered strikingly more obvious by the interposition of a thin cloud —a circumstance which I have actually observed.

But apart from inequalities of light which are referable to the moon itself, there are other inequalities which are unquestionably proper to the earth's shadow; or, as I have expressed it, there are variations in the amount of light in different parts of a transverse section of the shadow. I believe that most of these latter variations may be comprehended in the single statement that the amount of light increases from the centre of the shadow outwards to the edge. In the eclipse of January last, at the time when the moon was near the centre of the shadow, careful observation showed that the marginal parts of the moon were brighter all round than the more central parts. Much more obvious was the gradually increasing glow of light, towards the close of totality, on that part of the moon's edge which was about to emerge. This, indeed, is always an interesting feature in a total eclipse. I believe it is an invariable rule that whenever the moon is at all near the edge of the shadow, a marked increase of light may be observed on the parts which are nearest the edge; and even when the moon is not very far from the centre of the shadow, it is generally possible to judge, by the distribution of the light, on which side the centre lies.

I have never seen a satisfactory explanation of this. In attempting to explain it, I would first call attention to the fact, obvious on inspection of the diagram, that the solar rays which strike the *edge* of any given transverse section of the shadow are the same rays which strike the *centre* of a transverse section further down the shadow. They are, therefore, in accordance with a law already explained, rays which have undergone a less degree of refraction than those which strike the centre of the given section. Being rays which have undergone a less degree of refraction, they must be rays which have passed through a higher and more transparent atmosphere, and therefore they will be brighter rays. Deviation from the centre of the shadow is, in fact, in this respect, equivalent to increased distance from the earth; and just as the eclipsed moon in apogee should in theory be brighter than the eclipsed moon in perigee, so, it might be thought, should the parts of the moon nearer the edge of the shadow be brighter than those nearer the centre. I have no doubt that to an observer on the moon near the edge of the shadow the luminous ring surrounding the earth would, from the causes I have explained, be specially bright in the part corresponding to his own position. But it does not seem to follow that the region of the moon occupied by the observer would be specially illuminated in consequence; for while the part of the luminous ring corresponding to his position would be increased in brightness, every other part of the luminous ring as seen by him would be proportionately diminished in brightness by reason of the rays which would reach him from those parts being necessarily more refracted rays, and therefore rays which had passed through a denser and less transparent medium. Thus it seems very doubtful whether the total amount of refracted solar light falling upon an object just within the shadow would be at all greater than the amount falling upon an object near the centre of the shadow in the same transverse section.

Driven, therefore, to look for another explanation of the increase of light towards the edge of the shadow, I am disposed to seek it in the solar corona. Whatever difficulties there may be in admitting that the light of the corona can sensibly illuminate an object deep in the earth's shadow, there can, I think, be no doubt that the corona must throw a very appreciable amount of light for a short distance within the margin of the shadow; for here, it must be remembered, rays will fall from the very brightest parts of the corona—those immediately contiguous to the sun. The rapid diminution of light which we observe as the distance from the edge of the shadow increases is just what might be expected from the rapid diminution in the brightness of the corona as the distance from the sun increases. It is possible that the spectroscope might help in this particular inquiry.

Reports of Meetings.

GENERAL.

IN the 26th Annual Session, which has just ended, there have been eight General Meetings of the Society, all held in the Physiological Laboratory of University College, at 8 p.m.

On Thursday, Oct. 6th, 1887, Prof. Lloyd Morgan, F.G.S., read a paper on "The Stones of Stanton Drew," an abstract of which will be found at page 261.

On Nov. 3rd, Mr. H. Charbonnier gave an account of the "Bank Vole," and exhibited a living and some stuffed specimens. Dr. Shingleton Smith read a paper, entitled "The Structure, Decay, and Preservation of the Teeth." This will be found, in abstract, at page 286.

At the 3rd meeting of the Session, on Dec. 1st, Mr. H. Charbonnier showed a *white* specimen of the common Snipe (*Gallinago Gallinula*), recently shot in County Kerry. Dr. A. J. Harrison read a paper upon "Seals, and their so-called Ballast Bag." This is printed in full at page 290. Prof. Sydney Young, D.Sc., also gave a lecture upon "The Effect of Pressure upon the Boiling-point of Water, the Volatilizing-point of Ice, the Melting-point of Ice, and the Point of Maximum Density of Water; also the Relations of Pressure, Temperature, and Volume of Liquids and Gases." A full account of this will be found at page 298.

On Jan. 5th, 1888, Mr. G. Munro Smith, M.R.C.S., L.R.C.P., read a paper, entitled "Wine-drinking, and its Effects on the Human Body." This is printed in full at page 329. Mr. S. H. Swayne, M.R.C.S., then showed a ripe fruit of the "*Pyrus Japonica*," picked in his garden a month previously.

At the 240th General Meeting, held on Feb. 2nd, Col. Arthur M. Jones gave a lecture on "The Hybridization and Crossing of Ferns." This is printed at page 339. Mr. Alfred E. Hudd, F.S.A., exhibited a "Small Cabbage Butterfly" (*Pieris Rapæ*) caught on Jan. 29th in Clifton Vale. This species rarely emerges from the chrysalis before the end of March.

On March 1st Mr. C. K. Rudge, M.R.C.S., L.R.C.P., read a paper on "British Shore-fishes and their Habits." Mr. H. Charbonnier then exhibited two preserved specimens, viz.—(1) an almost white specimen of the common Greenfinch, and (2) a "Cross" between a Rabbit and a Hare, which had been lately shot in Yorkshire.

At the meeting on April 5th, Prof. Lloyd Morgan, F.G.S., read a paper on "Natural Selection and Elimination," which will be found at page 273.

The 26th Annual Meeting was held on May 3rd; and after the Report of the Council, the Balance-sheet, and the Financial Report had been read and adopted, Dr. Ramsay was elected an Honorary Member, and the officers of the Society for the ensuing year were appointed. Dr. Burder, F.R.Met.Soc., read a paper on "The Illumination of the Eclipsed Moon." This is printed in full at page 355.

PAUL A. COBBOLD,
Hon. Reporting Sec.

ARTHUR B. PROWSE,
lately Hon. Reporting Sec.

REPORT OF THE BOTANICAL SECTION.

THE genus Rubus has received continued attention, and during the season new localities for some of the rarer Brambles of the district have been recorded; whilst a few forms not previously clearly understood have been made out. In addition, one or two species entirely new to the Bristol Coal Field have been gathered and identified, through the courtesy of Prof. Babington. An account of these is not included in "Notes Supplemental to the Flora," published by the Editor, as the hope is entertained that at some future date the whole genus, in its relations to the Bristol Flora, may be overhauled and reviewed by the light of latest opinion and research. Since our last Report, there have been added to the list four additional species of phanerogams, namely, Erophila præcox, *Reich.;* Lepigonum salinum, *Fries;* Juncus diffusus, *Hoppe;* and Calamagrostis lanceolata, *Roth.*

JAMES W. WHITE, *Hon. Sec.*

May, 1888.

CHEMICAL AND PHYSICAL SECTION.

DURING the past Session four meetings have been held at University College, and papers have been read by the following gentlemen: Dr. A. Richardson, Mr. W. A. Shenstone, Dr. S. Young, Mr. D. Codrington Selman, and Mr. P. A. Cobbold.

The number of members is 35.

SYDNEY YOUNG, *Hon. Sec.*

ENGINEERING SECTION.

DURING the Session 1887–8 eight meetings were held. The following papers were read: "The Arch" (two), Mr. Charles Richardson, C.E. (President); "The Setting of Steam Boilers," Mr. C. J. Spencer; "Continuous Brakes," Mr. A. W. Metcalfe; "Tunnelling through various Strata," Mr. Joel Lean; "Transmission of Power by Sphere and Roller Mechanism," Mr. E. Shaw; "Some Indicator Diagrams taken from a Condensing Engine," Mr. R. H. R. Pope; "Future Engineering," by Mr. H. H. Simpson.

The members of the Society generally were invited by the President, Vice-President, and Committee of the Section to attend the meetings at which the papers on "The Arch" were read.

NICHOLAS WATTS, *Hon. Sec.*

ENTOMOLOGICAL SECTION.

DURING the summer months only one excursion was taken by the Section, to Dursley, on July 5th. A large number of species were taken by the different members, among them being Arge Galathea, M. Aglaia, L. Alsus, H. Linea, G. Obscurata, M. Euphorbiata, E. Trilinearia, D. carpophaga, botyshyalinalis, paudalis, lancealis, and cinctalis, and among the plume moths Osteodactylus, Phæodactylus, Galactodactylus, and many others, the excursion being one of the most successful ever taken by the Section.

At the indoor meetings of the Section a very large number of interesting species, both British and exotic, have been exhibited, the most singular being a hermaphrodite specimen of Odonestes Potatoria, one side being the male form, and the other the female; the male and female forms being so different gave the specimen a very peculiar appearance, the heavy plumed antennæ of the male being in strong contrast to the finer female form. Another very interesting exhibition was a hybrid between two of the large Sphinx moths, S. Populi and Ocellatus.

No papers of importance have been read during the Session.

GEORGE HARDING, *Hon. Sec.*

GEOLOGICAL SECTION.

TWO evening meetings of the Section have been held during the past Session, when papers were read by Mr. Joel Lean on the "Dolomitic Conglomerate of Bristol," and by Prof. Lloyd Morgan on "Elevation and Subsidence," respectively.

ROBERT A. CHARLETON, *Hon. Sec.*

PROCEEDINGS

OF THE

BRISTOL NATURALISTS' SOCIETY.

ENGINEERING SECTION.

"*Rerum cognoscere causas.*"—Virgil.

BRISTOL:

Printed for the Society.

MDCCCLXXXVII.

The President's Inaugural Address.

Read Tuesday, January 18th, 1887.

Gentlemen,—On the occasion of our former meeting in this place, when it was decided to form an Engineering Section of the Naturalists' Society, and you elected gentlemen to fill the various offices of Committee Men, etc., to the Section, you did me the honour of electing me to be the first President, notwithstanding my natural hesitation to assume that office, on account of, amongst other reasons, my defect of hearing.

I have now to return you my warmest thanks for the unanimity with which you did me that honour, and to assure you, moreover, that it is the more gratifying to me because I take it to be—in a manner, and amongst my neighbours—a recognition of me as the originator and designer of the large and almost unprecedented engineering work in this neighbourhood, which has acquired considerable notoriety on its completion of late.

You all know, of course, that I allude to the Severn Tunnel. I may add that I have been all my life engaged in tunnelling, having begun in the Thames Tunnel in 1837, and now ending in the Severn Tunnel in 1887; having myself cut the *first sod* of the Great Western Railway on the

Box Tunnel, and that of the Bristol and Exeter Railway on Cambridge's Batch Tunnel, in company with my old friend George Clarke, now of Dowlais.

Having got through this little personal matter, I will now proceed to discuss our own special objects in thus meeting together, though I must allow that it was at first something of a puzzle to me to decide what thread of connexion there could be between the parent Society of Naturalists and such an offshoot as the Engineering Section: what title they have, in fact, to claim the fatherhood of such an offspring.

I have only been able to satisfy my conscience upon this point by recalling to my mind that Naturalists are the disciples of Nature in all her grandeur, both animate and inanimate; and that the living animal is the highest study in Nature: for even the inanimate remains, and other evidences of a former existence that they have left behind them, form a distinctive branch of study in one of our most interesting and popular sciences of the day, and by the most learned professors of that science.

Now, I claim for the Engineers that their ingenuity has devised and constructed a most useful and powerful machine which makes the nearest approach to the *living animal* yet known, and this without being a mere copy or outside imitation of any animal: I mean, of course, the locomotive, or "iron horse," as it has sometimes been called, which, I take it, forms, as I have said, the nearest approach to the functions of a living animal ever yet made by man. It consumes its food and its drink, and dejects the refuse; and these operations are necessary for it to maintain its active powers. It breathes and puffs like a horse—more particularly while drawing a heavy load, or going up hill—and it is under the full control of its driver, like the best trained draught animal; and, lastly, it is equally useful as the servant of man.

These points of resemblance I maintain to be conclusive to my argument.

Many years ago, I recollect to have read that a very ingenious French mechanician, of the time of Louis Quatorze, I think, made an artificial duck which could swim about upon a large basin of water by means of its legs, and move its head naturally as it did so. It would stop to preen and apparently lubricate its feathers. It would take its food, and swatter in the shallow water at the edge of the basin, and, with French-like ingenuity, was made to discharge its dejecta: all, as the account said, in the most natural manner possible, and to the admiration of the king and his courtiers. I do not remember whether it could quack like a duck, though I should think that most characteristic melody could hardly have been omitted. But though this was considered at the time a most wonderful imitation of nature, yet it had to be *wound up*, and no doubt otherwise prepared for a second performance in the most *un*natural manner, by taking out the food it had swallowed, and replacing the imitation dejecta it had evacuated.

This was no doubt an ingenious and, what might be called a duck of an invention; but it was simply an outside imitation—it had no sort of life in it, and could do no work like our noble locomotive. The palm, therefore, must be given to our mechanical engineers of the present day.

Accepting this theory of connexion with our sponsors, the naturalists, as being in every respect satisfactory and sufficient for our purpose, I may now be allowed to crave your attention to a few further remarks on a no less important subject—namely, the object we have in view in assembling here this evening.

We assemble, then, to discuss various and special questions in engineering, both scientific and practical, with the object of enlarging our views, and of profiting by the practical experience of one another in every variety of engineering work. This is an object that should be attended with benefit and instruction to all of us.

The profession of the civil engineer at the present day embraces so many special departments that I suppose no *one* engineer is at home in all of them: that, in fact, like as it is in all other professions, every engineer must be more or less of a specialist, having had his thoughts and his practical experience concerned chiefly and more particularly in some one special department of the profession.

This, I have no doubt, we shall find to be the case amongst ourselves. But, notwithstanding this, we shall all doubtless have a good deal to learn from one another by reason of individual differences of view and of experience in every subject; and I will further maintain that an engineer in any one branch of the profession will be more likely to form a sound judgment upon an engineering question in any other branch, than one of the outside public, by reason of his practical training, and of his constant habit of deducing the cause from the effect; and that, therefore, when an engineering expert in any one branch details his views and experiences upon his own subject, he will have a more interested and appreciative audience even among those engineers who have not, like himself, made that subject a specialty, than he would in any general audience.

In the discourse that is to follow these remarks, on the subject of Chilled Iron, for example, you will no doubt have valuable practical remarks upon a very important and interesting subject, which will be of use and of interest to all of us; though, for myself, I have not had very much to do, except incidentally, with the special question, having only had to do, to any great extent, with ironwork in the form of roofs, bridges, and railway work in general.

In my own experience, I have found that ironwork is of great value in its proper place and when judiciously applied; but otherwise it sometimes has its faults. I think, for example, that in building bridges engineers have rushed too indiscriminately into ironwork, and that in many cases our successors may have to pay largely for it in repairs and renewals, and *that* before many years are over. My opinion is that engineers are exceedingly bold in ironwork, and exceedingly timid in brickwork and masonry, far beyond their relative merits; that, nowadays, too many iron-topped bridges of moderate span are erected for railway and such like purposes, when good brickwork or masonry arches might have been used in preference. I have three objections to these iron tops: firstly, they cost a good deal more money; secondly, they are, as a rule, abominably ugly; and thirdly, they are of very uncertain durability.

In talking upon the subject to one of our most eminent engineers a short time ago, he backed up my opinion by telling me that he had had to rebuild many iron bridges that he had himself built; and, among others, two viaducts, each containing a number of spans, and all within a period of some forty years only.

The cause of the defect in strength is also very difficult to find out in some cases, as it was in one of these viaducts. From the increased deflection under a passing train, they could perceive, on close observation, that the spans were getting decidedly weaker; yet they could discover no cause for it. To the eye and to the rule every plate in the bridge seemed as sound as ever. The bridge was well painted; no piece of iron had diminished in size, and no flaw could be seen after careful examination, yet the bridge had become much weaker; so, as a final test, a portion of a rivetted joint was cut out and examined. Externally the joint was perfect. The plates and the rivet-heads were all apparently tight and close; but when the joint was cut open for internal examination, it was found that the rivets themselves had been corroded greatly at the joint between the plates in the middle, so much in fact that they did not *there* retain half their original strength; hence the hidden source of the weakness that had been observed.

On the other hand there are, of course, cases of very limited headroom, where iron-topped bridges are a necessity, such as that for which I erected the iron-topped bridge for the Bristol Harbour Railway to pass over Victoria Street, where we had only sixteen inches of headroom; and there are cases of very wide spans, where iron may be the most suitable material.

With regard to bridges of large span, theories as to the possible limit of span in iron bridges have been promulgated from time to time, and it was reckoned that a span of 1,500 feet for a railway bridge *might* be accomplished in that material. But now, by using steel instead of iron, that limit has been extended in favour of the stronger material; so that we have at the present time a bridge in process of construction, across the Firth of Forth, of 1,700 feet span, three to the mile, an immensely bold conception.

Talking of *theories*, it is a common saying that "theory and practice don't agree." Now, I for one do not admit the correctness of this apothegm. I say, on the contrary, that theory and practice *must* agree, if the theory be only properly applied to the practice. If a wrong theory be applied, or if any fact be omitted from the theory, why, of course it does not, neither could it be expected to apply;

and I think it will always be found, in such a case, that something has been omitted.

I may mention a remarkable instance of this, which occurred when the Bill for making the Great Western Railway was in Parliament in the House of Lords. The opposition came from the London and Southampton Company. They retained the celebrated Dr. Lardner, amongst others, to give evidence for them against the Bill; and he selected the Box incline, of 1 in 100 for about three miles, as the strong point for his evidence.

You may remember that this was in 1835. Railways were then in their infancy; and Dr. Lardner had the name of being a great mathematician, as well as the editor of his then well-known scientific *Cyclopædia.* I was a pupil of Brunel's, and attended the committee-rooms in those days.

His evidence went to the effect, that the trains descending this incline, if at any time and from any cause the brakes were to fail (as in all human affairs they must be expected to do *sometimes*), the train would run down the incline with a constantly accelerated velocity, until, at the bottom, it would have acquired a speed of 120 miles an hour —a speed he thought their lordships would not be prepared to sanction.

The members of the Committee were all aghast at such evidence from such a man.

Now, the Doctor's figures were no doubt quite correct according to *his* theory, which was that of a body falling 160 feet *in vacuo;* but he altogether omitted the restraining forces of friction and air resistance. If he had taken these into his calculation at their true value, he would have found that the train would have got up to a speed of 56 miles an hour, at about which speed it is found that frictional resistance balances the force of gravitation: so that if a train enters the tunnel at a greater speed than that, it gradually falls back to that speed; and if at a less, it gets up to that, and at that speed remains.

Brunel was full of ingenuity; and in reply to this, he pointed out that the Doctor had forgotten to take into full account the items of air and frictional resistance: and in addition to this, he went that evening to a skilful carpenter, and had a very cleverly-designed wooden model made of a gradient of 1 in 120 on a railway six feet long, natural scale, representing this gradient running down out of a deep cutting at a tunnel mouth.

Now, it must be borne in mind that, on a length of six feet, the gradient would be only six-hundredths of a foot, or three-quarters of an inch; and I must mention here that Brunel had the art to make this model in such a way as to completely deceive the eye, by placing the line of rails between heavy cutting slopes falling steeply in the same direction. The deception is caused by the great contrast.

The model was made of white deal; it was beautifully finished, and quite true, as could be measured by a rule at the ends. It was delivered at the G.W.R. Parliamentary Consultation-room at half-past nine the next morning, and placed upon the table under a green cloth.

Presently Brunel came in with Harrison, the G.W.R. counsel, and others. The big thing on the table under the green cloth immediately took every one's attention; and Harrison said: "What in the world have you got there, Brunel, under that green cloth?" Brunel answered: "An exact model of the dreadful gradient of 1 in 100 which is to take us all to everlasting smash, according to Dr. Lardner. I wanted you to behold this thing of awe; but before I remove the cloth, I wish to make certain conditions: You are not to touch the model, but to inspect it from all sides as carefully as you please; and then to tell me, if you can, which way the gradient falls—from where to where, pointing it out with your finger." He then took off the cloth.

Harrison walked round the model, carefully inspecting it, according to orders; but having formed a decided opinion at the first glance—an opinion which was only confirmed by further inspection—he said: "It falls from there to there, of course; any one could see that." Brunel then gave him a marble, saying: "That will tell no lies. Now put the marble on the *centre* of the model, mind—not at either end—and see what the marble says." Harrison did so; and, to the great astonishment of himself and of all the others (except Brunel), it ran the other way. After getting over his surprise, Harrison exclaimed: "Why, I'd have bet a thousand pounds to one it fell the other way!" However, he presently rubbed his hands, and said: "I'll take a nice rise out of Joy (the opposition counsel) with this."

The model was then taken to the committee-room, and placed upon the table under its green cloth.

When the Committee had all assembled, and were eyeing curiously the green cloth, Harrison stood up and said: "Their lordships had all heard the alarming evidence given yesterday by Dr. Lardner respecting the Box gradient of 1 in 100. His ingenious friend, Brunel, had in the meantime made an exact model of this gradient as it would appear on emerging from the lower end of the Box tunnel. The gradient of the model can be proved to be perfectly accurate; but instead of being such a dreadful affair as the learned Doctor had represented it, their lordships would perceive that it was a very harmless affair indeed; and he would take the liberty to request his learned friend opposite to put on his best spectacles, and he would defy him to say which way the gradient fell." The cloth was then removed;

and Joy, thus appealed to, said: "Oh, any one can see which way it falls!" pointing it out. The marble was then handed to Joy, Harrison saying at the same time: "This will prove the fact without fail. Will my learned friend be kind enough to place this marble on the *centre* of the model, and not at either end, to show which way it does fall?"

Joy did so with full confidence; but to the surprise of all, Committee included, it again ran the wrong way. The astonishment was so great, and the deception of the eye so complete, that the members of the Committee tried it themselves over and over again, wondering at the marble always running, apparently, up hill. They thus found out that they could not, in all cases, believe the evidence of their own eyes.

These eye deceptions, as most of you doubtless are aware, are not very uncommon. For example: a Grecian column has to be bulged in the middle to make it *look* straight. If a straight horizontal tie rod in an iron roof is, from the position of the observer, crossed by two other rods at a wide angle with it, like a broad letter A, the tie rod will appear to be greatly bent at the points of crossing; and the eye cannot reconcile itself to this deception so as to make it look straight when you know it is so. I could adduce more instances; but these may suffice as an illustration of the fact.

And now, Gentlemen, I think I have occupied your time long enough in this discursory fashion; and I will, with much pleasure, make way for Mr. Morgans and his more pertinent discourse upon the subject of Chilled Iron.

Chilled Iron.

By THOMAS MORGANS.

(*Abstract of Paper read on Tuesday, January 18th,* 1887.)

AMONG the descriptions of chilled castings in common use, the author instanced the following :—sheet, corn-milling and sugar rolls, tilt hammer anvils and bits, plough shares, "brasses" and bushes, cart-wheel boxes, serrated cones and cups for grinding mills, railway and tramway wheels and crossings, artillery shot and bolts, stone-breaker jaws, circular cutters, etc. Mr. Morgans then spoke of the high reputation of sheet mill rolls and wheel axle-boxes made in Bristol. Of the former he remembered that so long as thirty years ago sheet rolls made by Messrs. Bush & Beddoe (predecessors of Messrs. Bush & de Soyres), of Cheese Lane, were most generally in use in the tinplate mills of South Wales. Of the latter, in combination with wrought iron wheels and steeled axles, the local Wagon Works Company are exporting large numbers.

The comparative hardness of good chill and that of the most highly hardened steel, used for say die stamping or chill cutting, was referred to. This is one of the abstruse parts of the subject. In the matter of a footstep for an upright shaft, opposite opinions are to be met with as to whether a chilled cast-iron or hardened steel step is the harder. In this instance other considerations—as comparative contexture of the rubbing surfaces in contact, tenacity, lubrication, etc.,—come in to affect the result. A tool that cuts chill must seemingly be harder than the chill itself, but in this case tenacity comes in as a factor. For laminating and sometimes punching *hot* metals, the nature of the hardness of chilled cast-iron enables it to hold the field. Obviously a hardened steel roll for a sheet mill would become worthless as soon as it approached an annealing temperature (ranging from 420° to 600° Fahr.) in work.

With respect to the strength and fatigue resistance of chilled castings, details were given of some impact tests made in July, 1864, at Pontypool, in the presence of Captain Palliser, upon some of his chilled artillery bolts, 12¾ inches long by 4 inches diameter, made from Pontypool cold-blast pig iron. Those made from No. 1 pig iron (the most graphitic and costly) broke more easily than those from No. 2; and so on, until those made from No. 4 were tested, when the maximum strength was reached. No. 4 pig iron was in fracture a pale grey, bordering on mottled. The bolts made from it chilled *throughout;* those made from No. 1 pig would not chill more than ⅛ inch deep. The same mould chills were used for all the bolts.

Several points regarding foundry operations in the production of chilled castings were raised for discussion. They embraced the depth of chill to be imparted to chilled rolls and railway wheels, and, in the case of traction wheels, the width of chill in the tread; preparation of the chills (by coating with various carbonaceous matters, lime, beer grounds, or occasionally some mysterious compost) and moulds; selection and mixture of pig irons; methods and plant for melting; suitable heat for pouring; prevention of honeycombing; ferrostatic pressure of head; "feeding," etc. Melting for rolls being mostly conducted in reverberatories, the variations in the condition of the furnace atmosphere, altering from reducing to oxidising, and *vice versâ*, in cases of bad stoking and different fuels, were referred to as occasionally affecting results. Melting being an operation involving appreciable cost, and the achievement of corresponding furnace effects in a series of meltings of a standard pig mixture being a matter of much moment, the question of the suitability of Siemen's Radiant-heat Melting system for this purpose was introduced for discussion.

For promoting the success of a chilled roll in its work, latheing or turning it to perfect circularity in the necks first, and then turning the body while the necks bear in steady brasses, are matters of the utmost importance. The author next referred to the great excellence for chilling purposes possessed by some American pig irons, and to the fact that iron of a given carbon content derived from some ores and fluxes may differ much in chilling properties from iron holding a similar proportion of carbon (free and combined) derived from other ores and materials. Those irons are best which develop the hardest possible chill most uniformly to the desired depth, without producing a too abrupt line of division between the hard white skin and the softer grey body.

The impossibility of securing a uniform quality and chemical composition in any number-grade of any brand of pig iron over a lengthened period was adverted to. At some blast furnace establishments the pig-iron product, owing to one or other of several causes that were mentioned, varies from day to day. In others the changes occur less frequently but with equal certainty. Consequent from this a too resolute faith in any particular make of pig iron is likely to be at times ill-requited.

Occasional physical tests, accompanied with chemical analyses of irons used for chilling were advocated, and the author was of opinion it would be well whenever a chilled casting had enjoyed a good reputation for standing up to its work, that when it was retired from service some portions of it should be chemically analysed, so as to obtain clues to compositions of excellence. Great advantage would ultimately arise from a comprehensive series of such examinations.

Some of the physical characteristics of chilled iron,as well as the surprising locomotive properties of carbon present in heated iron, were noticed. Attention was called to some German data, published by Dr. Percy in 1864, concerning an iron which before melting weighed (approximately), 448¼ lb. per cubic foot, and contained (approximately) 4 per cent. of carbon (3¼ being graphitic, and ¾ combined). The chilled portion of a casting from this had a specific gravity equivalent to 471 lb. per cubic foot, and contained 5 per cent. of carbon, all combined. The soft portion of the same casting weighed 447¾ lb. per cubic foot, and contained 3⅘ per cent. of carbon (3⅕ being graphitic and ⅗ combined). Mr. Morgans doubted whether so great an increase in density often arises from chilling. Tool steel,when hardened by being chilled in cold water, does not become condensed, but slightly expanded from the bulk it possesses when annealed or soft. Here an increase of hardness is accompanied by a decrease of density. The gradual development of a network of cracks over the face of a chilled anvil or bit being used in tilt hammers was mentioned. Such minute cleavages become more marked as the chill is worn down by work and from grinding. Traces of the same occurrence are observable over the surface of much-worn chilled rolls used in sheet mills. In such cases the sheets get a faint diaper pattern impressed upon them. The opening of crack spaces points to lateral shrinkage of the portions of chilled material they surround, and to some release from a state of involuntary tension. If this action is accompanied by some actual densification of the fissured chill, then we have a result that possibly conflicts with the example of condensation from chilling cited by Dr. Percy. After expressing a hope that a good trade in corn-milling chilled rolls may grow up in Bristol, the paper concluded with an inquiry as to the most economical and least troublesome means of either breaking up or melting worn-out sheet rolls, some of which occasionally consist of lumps of three tons weight.

DISCUSSION.

Mr. de **Soyres** had always remarked the surprising consolidation and absence of honeycombing in the bodies of broken chilled rolls. He did not think it was usual in this country to use a pouring head of so great a height (with corresponding augmented liquid pressure) as mentioned by the author of the paper. Where an open-hearth melting plant existed, the melting down of old rolls was not difficult nor expensive. He agreed with much that had been said as to the importance of pig mixtures and uniformity of melting.

Prof. **Ryan** referred to the chemical composition of chilling-iron mixtures as being of importance, and he advanced views to account for the cracking which develops in chilled surfaces subjected to heat during wear.

Mr. **Sutcliffe** drew attention to the comparative effect of radial and tangential "gates" for introducing the molten metal to the mould, and also to a method of sooting the interior of the chilled portion of the mould, and also to the *rôle* played by this sooty or other covering in, among other things, preventing the sticking of the casting to the mould.

Mr. Gross mentioned some remarkable results in working endurance evidenced by some chilled tramcar wheels made in the Gateshead district. He added that old beer grounds undoubtedly made a good wash for the surface of the chilling mould—probably because of their carbonaceous constitution.

In reply to Mr. Harvey, the Author of this paper said he was unacquainted with any exact experimental data referring to the tenacity of chilled iron. Hardening *steel* increased its tenacity up to a point with concurrent modification of its flexure resistance. The strength of the Palliser bolts which were chilled throughout was very striking.

On the Method Adopted to Compound a Pair of Ordinary Oscillating Paddle-wheel Engines.

By JOHN W. L. HARVEY.

Read on Tuesday, February 15, 1887.

BEFORE describing the method adopted in this case, perhaps it will give you a clearer understanding of the matter if I shortly state the conditions under which these engines were working prior to their conversion, and more especially as in the search after economy the history of these engines is somewhat unique if not altogether exceptional; and as I go on, you will have your attention directed to the fact of three separate classes of engines at work in the same hull, viz., "jet condensing," "surface condensing," and "compound," under almost identical conditions of "draught of water," "displacement," "midship area," "propeller," etc., from which probably we may be able to draw some useful conclusions as to the cost of motive power in steam vessels, having regard to the description of machinery employed.

Now, gentlemen, I am aware that the object of our meetings here is our mutual advancement, but in order that you may have a clear understanding of the several conditions under which these engines have been working, I must ask your indulgence if I take you so far back as the year 1868 in which year the vessel I am speaking of was built.

Her dimensions are as follows :—

Length	261 feet.
Breadth	29¼ ,,
Depth of hold	15 ,,
Gross tonnage	1,022 tons.
And her "draught of water" on trial was 9 feet 3 inches mean, giving a displacement of	1,172 ,,
And midship area of	221 sq. ft.
Co-efficient of Fineness	·576

She was then fitted with a pair of oscillating paddle-wheel engines of 350 nominal horse power, having 2 cylinders each, 66 inches diameter × 72 inches stroke; her condenser was on the jet condensation principle, and the vacuum was maintained at 25 inches by two single-acting air pumps, 42 inches diameter × 28 inches stroke.

The feathering paddle wheels were 23 feet 6 inches diameter over the outer edges of the floats, and each wheel was fitted with 10 floats 10 feet wide × 3 feet 8 inches deep, and at the above mean draught (9 feet 3 inches) the total immersion of the floats in both wheels was 220 square feet, giving ·187 square feet per ton of displacement, and just 1 square foot per square foot of midship area.

Steam was supplied to these engines at 30 lbs. pressure by 4 rectangular boilers having 16 furnaces, with a firegrate area of 350 square feet, and the total average consumption of good Welsh coal, to which I wish to call your particular attention, was 92 tons per voyage.

I am compelled to state the consumption thus broadly in terms of the voyage, as, owing to disturbing causes, such as "varying speeds," "strength of the tide," "waiting for water to enter the harbours," and "lying with banked fires," there is no reliable data available whereby the exact consumption per hour at full speed could be obtained without a special trial, for which there was no opportunity. But as this vessel has been uniformly employed upon the same service, this will be quite sufficient to enable us to compare the results so as to get at the economy of the three systems. I should state that the consumption in each case has been arrived at by taking the average for twelve months.

The rate of combustion is 15 lbs. per square foot of grate per hour, which was found to be the most economial rate with natural draught (for we have not arrived at forced draught in this vessel yet), and for obvious reasons this is the rate which has been maintained throughout.

Under these circumstances then, the mean pressure in the cylinders was $21\frac{1}{2}$ lbs. per square inch, the engines made 30 revolutions per minute, and developed 1,605 indicated horse power, giving the vessel a mean speed of $14\frac{1}{10}$ knots per hour, and a displacement coefficient of 194.

These, then, are the conditions under which the first arrangement of the machinery worked for eight years.

Then had to be realized the fact experienced by all who have charge of the practical working of engines and boilers, that whereas the engines were in a condition to still develop their maximum power, provided sufficient energy was applied to the pistons in the shape of steam at good pressure, on the other hand the boilers themselves were feeling the effects of old age, indicating that it would be prudent to allow them to retire from active service as soon as convenient; so that, in common with the fate which awaits mortals as well as machinery, they were gracefully retired, and eventually found a resting-place on the scrap heap.

This, then, brings us to the year 1875, and to the consideration whether the old state of things should be perpetuated by replacing the old boilers with new of similar construction, or whether by some means the efficiency of the vessel as to speed and cargo-carrying capacity could still be maintained, and at the same time the coal bill reduced, which is as you are aware the big leak through which the gross earnings of a steamer disperses itself; and I would take this opportunity of impressing upon you all, in these times of distressed ship-owners, with what readiness they would receive any one who would introduce to them a means whereby their coal bills may be materially reduced or done away with altogether.

I have great faith that electricity has a mine of wealth in store for us here. Cannot some of our electrical brethren come forward and help us?

However, I must not waste any more time in vain speculations, but return to the consideration of what is to be done with our boilers.

At this time scarcely anything was being built but compound marine engines, with boilers working up to as much as 100 lbs. per square inch, and consuming about one-half the coal per indicated horse power; here at once was a great field for a reduction in the coal bill, and there was considerable difficulty in resisting the temptation to discard the old machinery altogether, and replace it with other on the compound principle, had not the question of pounds, shillings, and pence obtruded itself into the calculation. How much more satisfactory to themselves it would be were engineers allowed *carte blanche* in these matters!

As it was, at that time, although it was seriously under consideration to fit the vessel with the compound machinery, competition was not so keen as to warrant such a heavy outlay as the doing so would involve; and although it was also under consideration how the existing engines could be cheaply compounded, it was found it could not be done in the space at disposal, and the same rate of speed maintained. So that the idea of compounding had to be abandoned, and recourse had to the only other plan of securing economy, by fitting the vessel with a surface condenser and smaller boilers working at the orginal pressure, whereby it was anticipated that a considerable saving, of twelve per cent., in the consumption would result by working the boilers with condensed water instead of sea water, thus obviating the necessity of what is technically known as "brining" the boilers, and at the same time in a great measure doing away with the expense of chipping and scaling necessary to keep the old boilers clean.

The new boilers were in consequence made with 14 furnaces instead of 16, giving 307 square feet of grate surface instead of 350 square feet as before. This reduction in the boiler capacity barely allowed room for a surface condenser having 4,754 square feet of cooling surface, and a 16-inch centrifugal pumping engine. The exhaust pipe of the cylinder of this engine was connected to the condenser, so that when the main engines were at work it consumed little or no steam, and the power to drive the pump was almost entirely derived from the vacuum in the condenser. At first the two large air pumps were kept going as with the old arrangement; but on trying the engines for a voyage with one air pump working only, there was found to be as good a vacuum as when both were at work. One of the air pumps was then permanently disconnected, and kept as a spare pump, which of course had the effect of relieving the engines of the power required to drive this pump; or in other words, transferred that power from the pump to the paddle wheels.

Very well, then, to sum up the conditions upon which this vessel entered on her second commission in 1875:—

Two cylinders, 66 inches diameter × 72 inches stroke, as before.
Working pressure, 30 lbs., as before.
New surface condenser, with 4,754 sq. feet of cooling surface, instead of the old jet condenser.
New 16-inch centrifugal pumping engine.
Four boilers, with 14 furnaces instead of 16 as before,
Paddle wheels, the same us before.
The draft of water, displacement, midship area, remaining the same as before.

The result was that practically the same horse power was developed, the same speed was maintained, and the consumption was reduced to $84\frac{1}{2}$ tons per voyage, equal to a saving of $8\frac{1}{4}$ per cent. As I have stated before, the saving expected was 12 per cent., but it appeared that this was too sanguine a view, and economy on the lower scale was not despised, as it meant close on 400 tons on the year's working. Very well, then, she went on saving 400 tons a year until this second set of boilers was worn out, in 1885.

And now I come more particularly to the subject matter of my paper.

In the interval I need scarcely remind you that competition had become much keener, and that this vessel had to compete with others in the same trade more recently built, and fitted with compound machinery; and it was perfectly evident that she would be out of the race unless a considerable reduction could be made in the coal bill. The vessel was too good and too great a favourite to be cast aside as obsolete, and the cost of new compound machinery would have been such that so great an outlay would certainly not have been justifiable. There was nothing left for it but to compound the existing engines in the best manner possible, providing this could be done at moderate cost, and at the same time such a rate of speed maintained as to enable her to hold her own with her newer rivals.

This, then, was the problem to be solved.

From the outset it appeared that, independently of the boiler, it would be necessary for economy's sake to retain as much of the existing machinery as possible; and to do this it appeared necessary to keep one of the existing cylinders as the low-pressure cylinder of the new compound arrangement; for had it been attempted to fit a larger low-pressure cylinder, it would practically have condemned the whole engine. The old cylinders were, as I have said, 66 inches diameter × 72 inches stroke, and developed 1,600 indicated horse power; and to have got this power out of a compound engine with the same stroke would have necessitated cylinders 48 inches and 86 inches diameter respectively, which by reason of want of space was out of the question. It therefore became necessary to so proportion the new high-pressure cylinder as to obtain the maximum power possible out of the old 66-inch low-pressure cylinder which it was proposed to retain.

Now those of you who are familiar with the construction of an oscillating engine, and have entertained the idea of compounding it, will agree with me that the first difficulty which presents itself is how to get the steam out of the high-pressure cylinder into the low-pressure cylinder,—the exhaust trunnion of the former being amidships, and the steam trunnion of the latter being at the ship's side.

I confess that here at the outset was a problem that fairly puzzled me, and for some time I could not discover any suitable means of getting over it.

We all enjoy a stroke of luck when it falls in our way, and after mentally turning the engines inside out and upside down, I experienced this pleasure whilst pondering over the problem, when it occurred to me, Why not turn the old low-pressure cylinder, that was to remain, round on its vertical axis, the piston rod, and so bring its steam trunnion amidships, and connect it by a short straight pipe through the condenser between the air pumps to the exhaust trunnion of the high-pressure cylinder?

Now I venture to say that in nine cases out of ten, if you tried to do this it would be found that the columns, frames, and details of the engine would not admit of its being done. However, fortunately for my scheme, I found on trial that the original designer had provided for such a contingency, whether intentionally or not doesn't matter; suffice it to say that this was practicable in this case.

The next problem was how to get the maximum power out of a compound engine having a low-pressure cylinder only 66 inches diameter.

The mean pressure in these cylinders under the old conditions was $21\frac{1}{2}$ lbs.; to secure the same result the boiler pressure would have to be about 125 lbs. above that of the atmosphere, a pressure considerably greater than it was prudent to adopt.

Now here it appeared that I had only got over the difficulty of the steam passages to be wrecked on the rock of steam pressures.

There was nothing for it but to adopt a more moderate boiler pressure, and consequently a lower indicated horse power and a corresponding reduction in speed.

For various reasons a boiler pressure of 80 lbs. above the atmosphere was fixed upon, and by putting in a more than usually large new high-pressure cylinder, and carrying the steam well on through the stroke, thereby get the greatest possible mean pressure in the low-pressure cylinder.

Working out this idea, and equalizing the power in the two cylinders, it will be found that a high-pressure cylinder of 40 inches diameter, cutting off at eight-tenths of the stroke, with 95 lbs. initial absolute pressure, will give about 50·4 lbs. mean pressure in the high-pressure cylinder and 18·5 lbs. mean pressure in the low-pressure cylinder, and would at a correspondingly reduced piston speed of 25 revolutions, equal to 300 feet per minute, develop 575 horse power in each cylinder, or a total of 1,150 indicated horse power, instead of 1,600 as formerly; and this reduced power would give a speed of $12\frac{1}{2}$ knots per hour at the original mean draft of 9 feet 3 inches and displacement 1,172 tons, with the original coefficient 193, instead of $14\frac{1}{10}$ knots as formerly.

Such an engine would require two boilers, 16 feet diameter × 11 feet long, with a grate area of 116 square feet, and a total heating surface of 4,500 square feet, and the consumption of coal I estimated would be 60 tons per voyage.

This appeared to be the best result that could be expected under the circumstances, and the scheme was eventually approved.

I will now shortly explain the alterations that were necessary to carry out the idea. The method of compounding these engines, then, was as follows:—

To remove the old starboard cylinder, and in its place to fit a new high-pressure cylinder, 40 inches diameter × 72 inches stroke, with a receiver cast round it in the form of a jacket, making the outside of the receiver about the same size as the outside of the present old cylinder. To have two slide jackets fitted as usual, but on the wing sides of the cylinder. To admit the steam from the boilers by a steam trunnion at the wing side as at present, and to exhaust it into the receiver surrounding the high-pressure cylinder and through its midship trunnion into a pipe or passage to be formed in the casting of the old condenser between the two air pumps. To reverse the remaining old cylinder and trunnion blocks, so that the steam trunnion may be brought amidships, and to take the exhaust steam from the high-pressure cylinder through the pipe or passage between the air pumps, and exhaust it through the trunnion at the wing side of the reversed old cylinder, carrying it thence by a pipe into the surface condenser, which was already fitted at the fore end of the engines; this will bring the slide valves on the wing side of this cylinder also. The same quadrants and valve levers will come in again, but will have to be removed from the midship columns to the wing columns in each case, and a new link motion to be fitted to each cylinder, with the eccentrics on the paddle shaft close to the outside of the wing bearings, where the original expansion cams were fitted. A new weigh shaft to be fitted across the after columns, to actuate these links. The starting to be accomplished by a new steam starting engine in place of the present hand starting wheels and racks, with starting valves to each cylinder; by putting the slide valves on the wing sides of each cylinder, the feed and bilge pumps will have to be shifted from the wing to the midship corners of the side frames, and new feed pumps would be required.

Two new steel cylindrical boilers would be required, 16 feet diameter × 11 feet long, having in all six furnaces, 45 inches diameter × 7 feet long, giving 146 square feet of grate; 572 3½-in. tubes 7 feet long, and a total heating surface of 4,500 square feet.

Thus producing a compound engine with cylinders 40 and 66 inches diameter × 72 inches stroke, working at 80 lbs. boiler pressure, and sacrificing the least parts of the old machinery,—one cylinder, the entablatures, shafts, paddle wheels, condenser, side frames, air, circulating, and bilge pumps, etc., being retained.

I do not think I need enter here more fully into the details, but I shall be happy to give any further information that you may desire later on.

These alterations were eventually carried out by the original builders of the vessel. It was the first job of the kind they had undertaken, and, as far as I know, such a method of compounding an oscillating engine has never been attempted before or since. The ultimate result was watched with interest and no little anxiety; and when all was completed, and the vessel proceeded to make her trial trip, was a very anxious time for all concerned—for it was a decided case of "making a spoon or spoiling a horn," and a very valuable horn too. However, much to everybody's satisfaction, when steam was got up for the first time, the engines went away without a hitch, and all being found in order as regards their working, the vessel was taken out on a trial trip, and made three runs on the measured mile, with the following results:—

Boiler pressure	79 lbs.
Receiver ,,	16½ ,,
Vacuum	25¾ in.
Revolutions	25
Piston speed	300 feet.
Mean pressure, high-pressure cylinder	55·14 lbs.
,, low-pressure cylinder	21·16 ,,
Indicated horse power high-pressure cylinder	628·95
,, ,, low-pressure ,,	670·11
,, ,, total	1,299·09
Mean speed of the 3 runs	12·638 knots.
Mean draft	9 ft. 9 in.
Corresponding displacement	1,217 tons.
Displacement coefficient	177

A run was then made between the Cloch and Cumbræ Lights, a distance of 13·666 knots, which was covered in 1 hr. 3 mins. 15 secs., equal to a speed of 13 knots, with a ¼-knot tide in her favour, the indicated horse power being 1,239·2, which gives a coefficient of 190·7.

Now, taking the mean indicated horse power at 1,269, and displacement 1,172 tons, corresponding to her original trial draft of 9 ft. 3 in., and using the original displacement coefficient, 193, you will find that her speed, if referred to these original conditions, would be 13·4 knots, against $14\frac{1}{10}$ knots formerly. So that with 336 less indicated horse power we lose just three-fourths of a knot in speed; or in other words, it takes 336 indicated horse power to get the last three-fourths of a knot out of her under these conditions, or about one-fifth of the whole power.

Now, in conclusion, when I am able to say that the present average consumption of coal per voyage is 49 tons, as against 92 tons formerly, I think you will agree with me that, notwithstanding the loss of speed, which fortunately could be spared, this drawback, if it may be so called, is fully compensated for by the enormous reduction in the consumption. But the whole of the saving in the working expenses does not lie in the reduced consumption alone; for in consequence of only having six furnaces to fire, instead of sixteen, she requires five less firemen than before, which with other incidental savings brings the total reduction of engine-room working expenses to close on 42 per cent. per annum, and this without impairing the vessel's efficiency for the trade in which she is employed.

It is interesting here to note that when working with a jet condenser the consumption per voyage was 92 tons; when working with a surface condenser the consumption per voyage was $84\frac{1}{2}$ tons; and when compounded the consumption per voyage was 49 tons;—a saving of $46\frac{1}{2}$ per cent. over the jet condenser arrangement, and 42 per cent. over the surface condenser arrangement. I must confess the actual economy exceeded my greatest expectations, and brings very forcibly before one what can be done by compounding. This result was deemed highly satisfactory, and has enabled the vessel to compete successfully with those of a more modern type, and to retain her place on the line in which, through all weathers and at all seasons, she has established for herself a favourable and widespread reputation.

I am afraid this bare statement of facts does not afford much ground for discussion, but there is one point in connection with this subject upon which I should like to hear your remarks, viz., How is it that marine engineers have been allowed to get so far in advance of the locomotive and stationary engine builders. For whereas marine engineers and shipowners have been enjoying the benefits of the compound system for the last twenty-five years, have in fact been getting their indicated horse power for an average expenditure of 2 lbs. of coal per hour, and even this system is now almost obsolete with them, for engines on the triple and quadruple expansion principle are rapidly displacing the ordinary compounds, giving an indicated horse power for an average consumption of 1¼ lbs. per hour; on the other hand our railway directors and manufacturers are still for the most part contented to give 5 or 6 lbs. of coal for their indicated horse power. Surely mechanical engineering should not be in the depressed condition it is in at present with such a field for economy before it.

If quadruple expansion was applied to our locomotives, it should result in the quadruple expansion of dividends, and shareholders should get dividends of 20 per cent. instead of 5 per cent., with which they seem now quite content.

Notes on Stationary Engines.

By G. W. SUTCLIFFE.

Read on Tuesday, March 22nd, 1887.

IN the following notes there is no pretence to originality set forth, but the hope is felt that perhaps a good discussion may be raised, as the subject is one which is more or less within the province of all engineers. In some measure the matter is extracted from a paper laid before the Institute of Civil Engineers some years ago.

As a rule there is no sort of boiler which for considerable powers will compete with a Cornish or Lancashire one. Small powers are, however, most cheaply provided by the upright or crane boilers; and where space is specially valuable, some kind of tubular boiler may be adopted with advantage. The grand advantages of the Cornish and Lancashire boilers are that there are no very narrow spaces to become choked up with sediment, and that there need be scarcely any part of the whole, inside or outside, which is not fairly accessible. Other advantages are that they will stand much abuse from an incompetent stoker with impunity, and that they are comparatively little liable to prime.

Now as to the narrow water spaces, it may be remarked that many large boilers have been built and worked, in which the spaces are much smaller than there is any necessity for, and consequently scale accumulates here; it is in such cases not uncommon to find scale of extreme hardness, half an inch or more in thickness,—but this is a condition of the past rather than of the present. Then as to accessibility, there is but little to say, except that the seating of all such boilers should be in the form of large fire-clay blocks, and that all those under seams or butts should be cut almost through by a saw before firing, so as to be easily snapped by a blow, when these blocks can be easily arranged so as to be withdrawn at any time, and the boiler most thoroughly examined; it may be here remarked that in good practice boilers are now never set upon a central midfeather wall, which has often set up a line of corrosion along each side, and which also acts against uniformity of heating.

The freedom from priming, which is a happy experience of the use of these boilers, is caused in a great measure by the ample water spaces which are so easily provided, and which allow the greatest scope to the currents set up by convection; the more or less complete absence of this facility in boilers of other types causes trouble of which the source is often unsuspected. The comparatively large steam space also tends to promote the production of dry steam, but in this respect perhaps it would be too much to claim special advantages, as many other types of boiler are well provided on this point. It may be here remarked, that if every boiler made were to be provided with a slit or perforated pipe for taking off dry steam, much trouble would be avoided.

Little need be said as to the material of which boilers generally are made, as this is becoming more and more exclusively steel instead of iron. Steel is now made so excellent and reliable in nature, that it may be treated almost anyhow, except at one particular temperature—at and about what is known as "black-red." Whilst, however, the material has been less reliable, it has been found to be necessary to drill all holes in plates exposed to tension; this is found to be attended with many advantages, and withal at such a low cost, that the universal practice of punching, which was followed with iron, and which is now possible with steel, will probably never be reverted to on a large scale.

All boilers should be fed with water which has passed through some kind of feed heater, as by this means straining of the plates owing to differences in temperature are avoided. Any feed heater which is used so as to take advantage of the waste heat in the chimney gases, or in the exhaust of a non-condensing engine, introduces an element of economy which is of importance; but another advantage is also secured in the fact that with almost all descriptions of water much solid matter is precipitated in the heater, and therefore before its entry into the boiler. There is, however, a great probability that before very long some sort of apparatus for the purpose of water purification or softening will come into extensive use. If this should be the case, there will be in all good practice two separate appliances brought to bear upon the water before its entry into the boiler, the first for the removal of impurities, and the second one for a preliminary raising of the temperature.

A boiler of the ordinary class, with two furnaces, each 2 feet 9 inches wide, and a good chimney, will easily burn twenty tons of coal per week's work of fifty-four hours, and evaporate 160 to 200 tons of water in the same time. Macfarlane Gray's approximate rule for marine boilers, is that one ton of coal is consumed per foot in width of the grate per twenty-four hours, irrespective of the length from front to back; the above figures for a land boiler give about half as much more, which is chiefly due to the better draught available ashore. Chimneys ashore are usually much taller than the funnels of steamboats, and of a substance which does not so readily allow the cooling of the contents; consequently the heated column on which the draught depends is both hotter and longer, so as to be in a double sense more effective.

Some years ago it was comparatively seldom that a feed pump was provided in a boiler house, the boiler feed depending on the engine feed pump, or at most a rough sort of donkey pump was provided for use when the main engine was not at work. More recently, however, there are so many donkey pumps in the market of good design, that it appears to be an open question whether or not engine feed pumps should be altogether abandoned. By this means one great risk is avoided, which is often a serious one in connection with an engine driven feed pump, this is that the feed valve is sometimes screwed down when the pump is working, when, if there is no overflow valve, or if this valve is out of order, a burst pipe is one of the least serious things which can happen. If a separate donkey-pump is adopted, this risk is entirely avoided, as any appreciable obstruction causes the complete stoppage of the pump. The stoker ought to be able to attend to a donkey-pump as well as to a feed valve, so that no objection need be raised on this score.

There is one feature which does not receive anything like the attention which it ought to receive in connection with almost all pumps and water-pipes. It is in the fact that unless the pipes, etc., are throughout exposed to a moderate and fairly uniform pressure, all possibility of lodgment of air in any part must be avoided. Almost all the mysterious troubles encountered in the working of feed pumps may be traced to this one point; there is some space where air may accumulate and not get away, and this air is exposed to alternate condensation and rarefaction. The avoidance of this trouble may, in rare cases and on long lifts, lead to some extra noise in the valves, unless they are carefully designed; but on the whole, the gain is a really important one.

Steam being raised in the boiler is expended in the engine, and this brings us to what is perhaps supposed to be the chief part of the subject.

The enormous variety in engines which obtains in the world cannot be realised without some consideration. There are horizontal, vertical direct, vertical invented, diagonal, beam and other types, also high and low pressure, condensing and non-condensing, simple, compound, and triple, and each of these exists in innumerably modified forms. The most common form is, however, the horizontal, which for large powers is almost invariably compound. Thirty years ago, almost all large engines were built of the old beam pattern, which though a costly arrangement, has never yet been equalled for comfort in working, and for easy access all over; in fact, neglecting the one element of cost, it is almost impossible to hope that any mechanical contrivance will ever be invented which will be more perfect than a well-built beam engine as arranged and perfected by Watt about a century ago. In comparison with other types, however, the engine itself is a very costly one, and the foundations and other preparations still more so; true, some economy has been effected by the use of A frames under the beam centres and in other ways, but these means only reduce the difference, and do not change its character. Of the old beam engines in use at present, there are comparatively few which are not compounded, and usually under the system of McNaught. In this case the high pressure cylinder is placed under the arm of the beam, to which the connecting rod is attached, and at about half way along this part, so that its stroke is about equal to the radius of the crank pin. This relieves the strain upon the beam centre in a most important degree, and for this reason has been sometimes resorted to, even where the attendant economy was quite a secondary matter.

In heavy driving ashore there is usually a great tendency evinced towards very long strokes, six and seven feet being not uncommon. This appears to be a mistake, as often the speed assigned does not reach forty revolutions per minute, and under these conditions it is very difficult to insure such steady driving as is often demanded; in fact, it may be said to be practically impossible with any great degree of expansion in one cylinder, and consequently the compound system is resorted to whenever any attempt is made towards economical working. The conditions imposed are not very unlike those obtaining in a paddle boat, as described at the last meeting. In both cases the engines are of necessity heavy, slow-working ones, and in both cases uniformity or steadiness in driving is a leading object. In each case an economical and satisfactory result is arrived at by compounding, though regarded simply from the point of view connecting coal bills and actual power obtained, it may be possible that better results would be obtained by expanding in single cylinders to the same extent. As a rule it is not the practice to adopt an early cut-off in compound stationary engines, exceedingly common conditions specified being that the lap shall equal the port, and that the port shall just open wide; these give a cut-off at about three-quarters stroke. Under these conditions, many engines are working on a consumption of 2 lbs. per 1 horse power, the bore of the low-pressure cylinder being double the bore of the high-pressure one, and the strokes of the two equal.

Hitherto there appears to be no cut-off motion actuated by the governor which is quite satisfactory and which is well received by all. The Corliss motion appears to be as good as any; in fact, but for the dictum of one leading authority, it might almost have been said to be a very good one, except for the reason that it is rather too delicate in its nature for an engine room. The question of variable cutting off is a very wide one, and one which has not received all the attention which it demands, notwithstanding the multitude of patents brought out and applied. Perhaps much of this neglect is due to owners, who require an earthquake or a prolonged coal famine before they will do much. It should however be borne in mind, that in almost all cases the coal bill is only a small part of the total expenditure which is necessary to any particular undertaking. In railways, for instance, the expenditure on coal only equals about one twenty-fifth to one-thirtieth of the total receipts, and is only equal in amount to about, say, one-seventh of the sum paid in dividends; so that if the whole coal bill were annihilated, the dividend would only be increased by that amount. It is to be regretted that this should be so, and chiefly is it to be regretted for the reason that though the pecuniary loss is not an extravagant one, yet every ounce of coal burnt unnecessarily tends to bring nearer the time when this island will again be compelled to grow its own fuel, owing to the exhaustion of the coal-fields; or perhaps, still worse, have to import coal from some outside, and perhaps, *now* barbarian nation. This is a point which rests more particularly with those connected with stationary engines, rather than with marine or locomotive practice, as in these, there is as a rule, a certain amount of expansion adopted in practice. Neither is it as a rule the large stationary engines which are at fault, but rather the medium and small sizes, which in the aggregate come up to a very important total, and in which, for one reason or another, there is an incredible amount of coal frittered away to small purpose. A certain amount of expansion is good in practically every case which has any pretence to good practice; but whether or not this should be variable, and if variable, whether or not it should be actuated by the governor, are questions to be decided in connection with each case separately. If the work is very uniform, the expansion may be permantly adjusted. If the work varies at certain considerable and well-defined intervals, the expansion may be adjusted by hand, and usually it is to be preferred that this should be done outside the steam chest, while the engine is running in its usual manner; in each of these cases a throttle valve of some sort, actuated by the governor, is a necessity. If however the work is liable to vary at any moment (and more especially if the variation is one of increase upon what may be spoken of as the ruling horse-power), then a cut-off motion, actuated by the governor directly, and without the intervention of the driver, becomes an essential condition in good practice. If sudden reductions in the required power occur for only short periods, then the governor and throttle valve are quite sufficient from any point of view. In speaking generally on the question of economy in coal, while as before stated, it is of immense importance, yet a practical audience need not be reminded that every case must stand upon its own merits, and due allowance must be made for many elements which cannot just now be discussed.

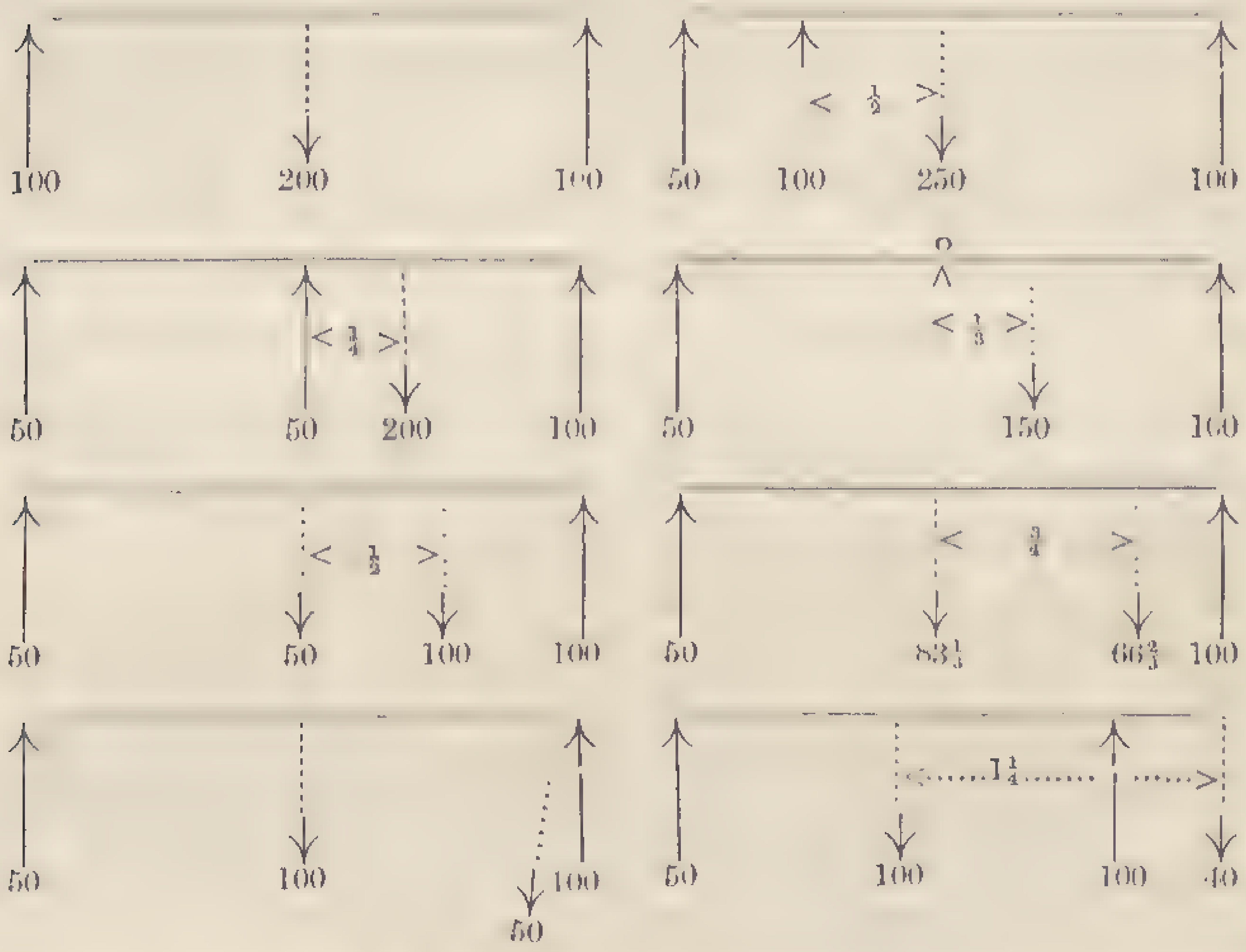
In all engines there is one element which furnishes a good index as to the mechanical design as a whole; this is the speed at which the engine can run with perfect safety. It does not follow that the economical properties of the engine would keep pace with the mechanical ones; but there is no question as to some points of superiority, and even indirectly of simplicity in a high-speed engine. In the first place, a smaller and more compact engine will do the work at a high speed; secondly, it becomes practicable to adopt a higher grade of expansion without recourse to the compound principle. It will also be found that the running is more uniform in high-speed engines than in low-speed ones. It need not be feared that a high-speed engine will "knock itself to pieces" with great rapidity if well designed and constructed. If, however, any one sets up a high-speed engine, relying upon its smaller size for a lower cost, he will find himself wofully mistaken. All machinery to be driven at a high speed must be throughout of the most unexceptionable quality, or its utter and complete collapse is certain; but granting this condition, its life is almost everlasting. The Allen engine of twenty years ago furnishes a case in point. It was introduced into this country by Mr. C. T. Porter, an engineer of the first order, who held that no part of an engine should wear out if properly designed; this, though not absolutely sustained in practice, has been so nearly approached as to raise hopes that perhaps at some time in the future it may be realised. Two of these engines were working for years, often up to one hundred hours per week, and with comparatively little trouble, and another had the scraping marks under the crosshead as plainly visible after ten years' work as they were on the day they left the shop. All these engines were working at 1,000 feet piston speed per minute, which is about the extreme reached in locomotive practice. Previously to the advent of the Allen engine, the usual speed in good practice for stationary engines was about 400 feet per minute. That the Allen engine did not meet with a much better reception than was the case, is a point for the most sincere regret, not only for the sake of the engine itself and those connected with it, but also in the general interests of steam-engine practice. As it was, its influence was not inappreciable, as speeds have been slowly but steadily rising ever since, and probably would not at the present moment have been even at their present pitch but for the Allen engine. Mr. Porter himself will, however, be better remembered in connection with his own governor and with Richard's indicator, rather than with the engine. Before leaving the question of high speed, it may be remarked that a thoroughly balanced condition all over is essential to the success of such machinery. All revolving parts must be balanced in detail; in other words, any disturbing element must be met by a compensating one, revolving in its own plane. It is not in the least degree sufficient that a standing balance be insured, as two wrongs cannot make one right, and really the addition of the second one (so-called balance), aggravates rather than relieves the trouble; this principle is of common application to all machinery in good practice. In steam-engine work another element is introduced, but one which usually does not receive the slightest attention; this is the balancing, or rather the adjustment, of the reciprocating parts. It is commonly assumed that all reciprocating weight is injurious in its effect on the running of an engine, and that consequently, if piston rods, pistons, connecting rods, etc., could be made of sufficient strength and without weight, an important gain would be secured. This may be true if no expansion of steam be allowed, but in any other case is a fallacy. In an engine working on a high or moderately high grade of expansion, and with comparatively light reciprocating weights, there is a great tendency for the engine to run more quickly during the first part of the stroke than it does afterwards. This is in a greater or less degree met by weighting the reciprocating parts. The mathematical expression of this principle is a very simple one; it is not however necessary to go into it here, but any one interested may find it in Porter's book on the "Richard Indicator," in *Engineering*, and elsewhere. While on the question of engine-balancing, it may be remarked that great advances in this respect have been made in locomotive practice during the memory of many of those in this room, and this success may be instanced in disproof of the principle just now referred to, viz., that all balancing should be effected in the plane of the disturbing element. In locomotive practice, it must however be remembered that the balancing is effected in the wheels to compensate for the cranks, big ends, etc.; when however the combined effect of the two balances is regarded, it is found to fall in the centre of the engine, and consequently the correct principle is followed with a closer degree of approximation than at first appears. This principle of splitting the balance into two equal and opposite parts has often been practised with good results; one case which may be mentioned is that of Mr. J. Bourne, who designed and introduced a specially good and compact engine about 1874, but which failed to take with owners. In this engine another point was tried, and perhaps this might have much to do with the failure; it was simply the rating of the article by the indicated horse-power, instead of the nominal. This afforded a better comparison with other engines as to the capacity for work, but it was open to the objection that owing to the fact that other makers of small engines did not follow the same custom, many corrections were necessary. It had also a tendency to appear to the ordinary buyer of small engines to be a thorough imposture. It is however unnecessary to remind the present audience that the rating of marine engines is becoming slowly but surely to be given in actual or indicated horse-power, rather than by the nominal, which it is to be hoped will become obsolete in a few years; in fact, there is some probability that if the Board of Trade or Lloyd's were to make a dead set against the "nominal" horse-power, its days would be soon numbered.

The question of horse-power reminds one that it would be well if every engine of size were to be thoroughly tested as to its power, economy, and capabilities generally. And this should be done in such a manner as to distinguish as to the capabilities of the boiler quite separately from the engine, and of any feed-heater (which may be in use) quite separately from either. This is comparatively seldom done, though it has been shown, over and over again, how it may be done with advantage. To class the whole set of engine, boilers, etc., together, and to speak of the economy of the whole, is much like classing together several steam vessels or locomotives, and when any defect is suspected, taking no steps to separate the several performances with a view to localise the trouble. Several private firms and boiler insurance companies now make a practice of testing under a proper system, amongst the foremost of which may be mentioned Messrs. Donkin and Messrs. Simpson, of London. Such testing, if generally adopted, would be the means of disposing of many pet theories as to the economies resulting from particular modes of working, which, though perhaps usually beneficial, are yet often very much overrated. For instance, a simple calculation will show that about 20 per cent. of economy is about the most which can be obtained without actually raising steam in a feed-heater, and this can only be secured by most scrupulous attention to detail throughout; it is not however uncommon to hear of claims ranging up to 30 per cent., or even more, on account of some such arrangement. This often results from a stoppage being necessary when any great change is made in working arrangements, and the opportunity is taken to give things a most thorough overhaul; the leading point in the project then often gets the whole credit, even when (as is the case occasionally) in itself it may be worse than valueless.

In conclusion, it may be pointed out that in the beam engine the cylinder stands vertical, with the piston rod out at the top; then in course of time this is displaced by the horizontal form, with the piston rod out at the end; and if it were not dangerous to "prophesy, except you know," it might be predicted that in the course of a few more years the ruling type for engines for rotative driving will be the vertical inverted, with the piston rod out at the bottom. This type presents several points of convenience. The crank shaft, be it ever so well balanced, still requires a good deal of steadying, and in the vertical inverted engine this can be done with the utmost facility and security. Then the whole of the strains are as a rule vertical, and therefore there is no excuse for elaborately divided brasses to carry the crank shaft. The floor space is less in these engines than in any other; and if there is any difference between the types in steadiness in driving, the vertical inverted engine will not suffer in the comparison. One other point may be suggested in connection with the engine of the future, viz., that the crosshead slides will be a thing of the past, and that a parallel motion will be adopted in its place. In the present marine engine there is a very important first instalment towards the parallel motion, in the form of the present pump levers, which would form part of such motion with a little modification. Perhaps after this the revolving wheel of fashion will again bring round the beam engine and the horizontal engine to displace the vertical inverted type, but at all events such is not the opinion held by your humble servant.

Replies.

The troubles referred to in connection with pump valves appear to have been misunderstood. For this defect of clearness an apology must be entered. The occasional noise in the absence of air was referred to, but the real trouble is that with objectionable air spaces the pumps will not draw at all; on long lifts and with warm water this is an uncomfortable fact which will bring itself home. There is not the least objection to the presence of air, but a great one to its accumulation.



The diagrams show that the correct balance in a compounded beam engine, when the work done is equally divided between the two cylinders, is attained by placing the high-pressure cylinder at one-third of the distance from the main centre to the connecting rod centre, neglecting the weight of the beam, etc.

The foaming referred to by Mr. Harvey gives an interesting illustration of the principle of convection referred to at the last meeting.

The perforations in the anti-priming pipe may be in any convenient position, perhaps rather better on the top; but in this case the pipe should be set down from the top of the boiler a little way, to avoid grooving the shell by the currents of steam flying past.

There need be little fear of trouble with a parallel motion, the total frictional resistances are smaller than those appertaining to slides; and so far from small surfaces (*per se*) giving most trouble, there is no joint in an engine which does as much work and with so little trouble as the "little end," to use a locomotive engineer's expression. One point in the parallel motion was not referred to, this is the varying character of the strains when a parallel motion is used for guiding and also for working pump.

Mr. Newall further explained the principle laid down as to the use of the cut-off principle. A variable cut-off may be in an extreme case adopted, and yet not save as much coal as was consumed in its construction.

The instances given by Mr. Morgan where long strokes are perferable, viz., to winding engines, are quite in order and most interesting; but this preference does not affect their acknowledged disadvantages in connection with constant and unremitting steady driving. The writer was at one time so far familiar with certain long-stroke engines as to be able to say at a distance of a quarter of a mile whether the correct or usual speed was being worked; this would obviously be impossible if high speed was adopted and correspondingly short strokes. This raises the question again as to what must be considered as high speed, and it may be remarked that a piston speed of 1,000 feet was thought of, with stroke of from three feet down to eighteen or twenty inches. One or two speakers seem to have had a greater number of revolutions in view; for instance, when reference was made to the Corliss gear proving unsuitable for such speeds, or by three cranks, whether triple or compound.

Several single-acting engines, with three or more cylinders, are in use, but two, as referred to by Dr. Ryan, would not be so efficient.

The bump referred to by Mr. Spencer, at the bottom of stroke of vertical inverted engines, will probably be largely got over by higher speed, or by three cranks, whether triple or compound.

The wheel draught referred to by Mr. Newall is instructive, but all the same he will excuse the preference expressed in the paper. Perhaps Mr. Spencer will one day favour us with an account of the system of boiler-setting referred to by Mr. Newall. The writer remembers a case many years ago showing the objectionable corrosion, which is usually charged against them, but how far this is explained by the old-fashioned use of common bricks and lime mortar, it is now impossible to say.

The locomotive coal bill includes the carriage and delivery of coal, and in all good locomotive practice expansion by notching up is resorted to, and the high speed adopted reduces the necessity for compounding elsewhere felt.

Several gentlemen referred to the large number of subjects covered more or less completely in the paper, and to the impossibility of discussing the whole; this being the case, it follows of necessity that a hope may be allowed that a reference to these heads will furnish material for many future meetings.

In conclusion, an expression of gratitude is called for on the account of all who have entered into the discussion. This is none the less urgent on account of the fact that much of the criticism was in direct opposition to the statements or opinions expressed in the paper, as it is to be clearly remembered that this is the only kind of criticism called for as a rule, except to support a point which has been previously attacked. At the same time, it is believed that others might have joined in with advantage, as every item is useful in one way or another, and as a rule it will be found on discussion, that in the majority of cases the differences of opinion which exist and which are appalling in their magnitude, will be cleared away and absolutely disappear.

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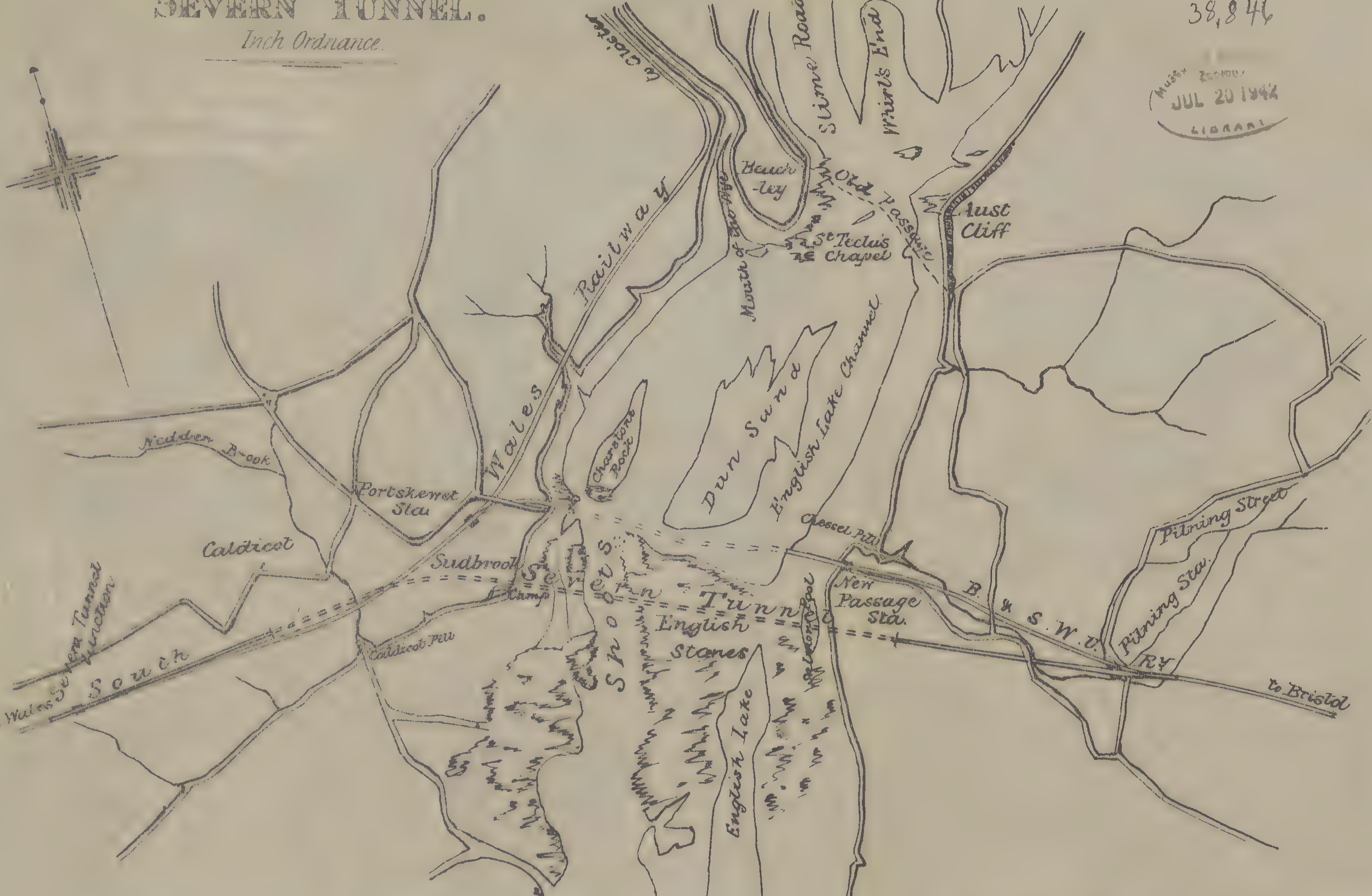

The double line shows Shore Line at High Water
The single line at Low Water.

The Severn Tunnel.

BY CHARLES RICHARDSON.

Read on Tuesday, June 28th, 1887.

THE first conception of making a Tunnel under the Severn came into the writer's mind in 1862 or 1863, at the time when he was constantly engaged on the Severn, during the erection of the New Passage Ferry piers, which were then being constructed for the Bristol and South Wales Union Railway.

The idea having once entered his mind, he could not help being struck with the different local facts, which all appeared to have a most favourable bearing on the scheme. The facts were these:—At the point where he proposed to cross, it was manifest, in the first place, that, out of a high-water breadth of $2\frac{1}{4}$ miles, two miles of the river bottom were dry at low water, and could be walked over. Secondly that the strata along these two miles of low-water rocks were horizontal and of a water-resisting character, and altogether most favourable for tunnelling purposes. Thirdly, that the deep channel of the Shoots had been cut through a very hard and close rock. And, lastly, but not least in importance, that the Severn waters are heavily charged with mud, and also—as the writer had the means of positively knowing, and will presently show—with very large quantities of solid matters, varying in size from a large and coarse gravel downwards to fine gravel, sand, and mud—due, no doubt, to the great rise and fall of the tides in this river, which amount sometimes to fifty vertical feet in one tide, and to the powerful currents thereby generated.

The portion of the river Severn near the New Passage Ferry is very remarkable because of these half-tide rocks, which end abruptly at the Ferry channel, in a line nearly square across the river. These rocks, called on the English side the "English Stones," reach all the way across the river, with the exception of the Shoots channel; and their abrupt ending is one cause of the existence of a Ferry channel, the other being the peculiar set of the tidal currents there. They are also the reason why all the Severn water *below* half-tide level has to pass through the Shoots channel, which is only 400 yards wide; for there is then no other outlet.

All these waters rushing down this narrow channel of the Shoots, naturally cause a very powerful current (ten knots an hour, sometimes), which no doubt gave this channel its name. Again, the Shoots channel, as you ascend the river, points towards the Welsh shore, so that the *in-flowing* tide presses against the Welsh shore; while on the other hand, the main stream of the *ebbing* tide is flung towards the English shore by the projecting point of St. Tecla's Chapel Rock, thus leaving an intermediate sandbank, called the Dun Sand, in the middle of the river, between the up and the down currents. The ebbing tide, thus thrown on to the English side, passes over the English Stones so long as the tide is high; but as half-tide is approached, the waters, having no longer an outlet that way, rush along the head of the English Stones down to the Shoots, and thus have to cut through the great bank of sand which had been deposited there by the flood-tide, and at that time formed part of the Dun Sand. This is the reason why deep water is always to be found there for the Ferry Passage. This process is repeated during every spring-tide, as the writer had reason to know; for on one occasion, in measuring and sounding across this channel, the bow of the boat was run aground upon the Dun Sand: the man at the bow jumped out and shoved the boat off again. In doing so, he made *two* steps on the sand; but the third, from which he meant to spring on board again, went into deep water, and he would have disappeared altogether, had he not held hard to the boat. It was found that the sand was being washed away so fast that it formed an upright face deeper than the length of an oar. This fact was afterwards frequently observed during ebb-tide. The Ferry channel there is washed to a depth of 42 feet below the lowest water of a spring-tide.

The Dun Sand, without doubt, joins on to the English Stones during the flow of the big spring-tides, and the Ferry channel is washed clear again by the English lake current every time at half-ebb. This proves the enormous quantity of *sand* that is moved during every one of these big tides.

Then, as to the quantity of gravel moved during such tides, there was a good illustration when the foundations of the Portskewet Pier were being got in. A base of brickwork, four feet deep, had to be put in to form a footing to which the framed wooden legs of the viaduct were to be bolted. When the men got to those near the low-water mark, it had to be put in during the low water of big spring-tides, and they found much trouble in excavating a level foundation for the brickwork out of the very hard marl, by reason of the large quantity of very coarse gravel that every tide washed into the excavation; so that some time was always lost in clearing the foundations of the gravel before work could be started again. The gravel nearly half filled the holes every tide, and was of a size sometimes equalling that of a man's double fist.

Now, as these holes were well above the low-water mark, while the Shoots channel in the main current is seventy feet lower, and as the bulk of the gravel, particularly the larger stones forming it, must run in the deepest channels, it was evident that *very* large quantities of heavy gravel must run up and down these channels with every spring-tide.

It thus became clear to the writer of this paper that, supposing only the strata were hard enough to resist the scouring action of the water, any joint or fissure in the rock must be choked with this gravel, sand, and fine mud so completely that no *Severn* water could possibly get down to any workings below, and that, therefore, the deep low-water channel of the Shoots—which is, without doubt, in rock more than sufficiently hard for the purpose—might be tunnelled under with perfect confidence. This has since been entirely borne out by the facts, and there is now positively *no* leakage of *Severn* water.

Having thus convinced himself of the perfect practicability of a Tunnel under the Severn, the writer, at the time, mentioned his idea to Mr. Leonard Bruton, the very able secretary of the railway then being constructed. He told him that he thought a Tunnel might be made under the Severn for half a million, exclusive of the in-shore approaches. Mr. Bruton's reply was—"Then, that is the right scheme."

From that time the writer began to make further investigations, by taking very numerous soundings in the Shoots, and by putting down trial borings, all of which fully confirmed his previous observations. He was, of course, greatly encouraged in this by his friend, Mr. Bruton, and by the directors of the Bristol and South Wales Union Railway, who all highly approved of the scheme. It was at this time that he made the first section across the river with the spirit level; but before he alludes further to this, it may be of interest that he should describe more fully the nature of these half-tide rocks, over which he had to make the sections.

The English Stones, before alluded to, are a very remarkable series of indurated beds of new red sandstone, stretching across the Severn in this place at about half-tide level, and for a distance of about two miles *along* the river. The induration has been such as to have caused the beds in this place to withstand the wearing of the powerful Severn currents, and of the frequent storm waves, for all the time the river has run down this course; while the beds of the same new red formation, above and below, have been worn away far below low-water level—out of harm's way, in fact. These hardened strata stretched originally quite across the river from shore to shore, and have been cut through only in one place—namely, at the comparatively narrow and deep channel of the Shoots. The explanation of these facts may be this:—Looking back to the origin of the river, after the land had been raised above the sea level, the incipient Severn waters would seek the lowest channel in their way to the sea, and this channel may at this place have been on the site of the present Shoots channel. Now, wherever this deeper channel was, it must have been subject to wear of a different kind from that applied to what are now the English Stones, and the other collateral rocks, from the fact that assuming the rise and fall of the tides to be the same as they are now, the strong currents of the spring-tides would then, as now, roll up and down immense quantities of coarse gravel at every tide. This heavy gravel would naturally seek the deepest channel in its course, and there *saw* a gradually deepening channel, which would thus occupy the site of the original course of the stream, and which now forms the channel of the Shoots. The formation of this deep channel of the Shoots, the writer, therefore, imagines to be due to the constantly repeated sawing action of the *gravel*, while the collateral rocks were subject to the action of the *water* alone. It thus happens that at low water the rocks may be walked over from either side up to the Shoots a mile and a half on the English side, and half a mile on the Welsh side. It is remarkable, however, that the Shoots channel, as is now known, has been cut through the *hardest* part of these hardened strata.

Over the English Stones the walking is very bad, for the surface of the rock is extremely rough and full of water holes : it is, besides, entirely covered with slippery sea-weed. No speed can be made over them. The walking upon them is also attended with great danger if it should happen to be a little late upon the tide, which rises over them with great rapidity. Two of the writer's assistants and their staff-holder had a very narrow escape from being drowned upon them about this time.

The only safe way of doing any work on these stones is to have a boat waiting at hand by which to get off before the tide is too high.

On the occasion referred to, the three had landed on the stones on the margin of the Shoots during low water of a good spring-tide, with their spirit level, etc., in order to start upon a section over the stones. The two men who took them there in the boat had orders to be there at the proper time to take them off again. Instead of waiting there, however, as they ought to have done, the boatmen pulled out into the stream, with the intention of rowing back again at the proper time; but the stream was too strong for them, and carried them down the river to Avonmouth, so that they could not get back in time; and when the levellers arrived at the assigned spot, there was no boat there, nor was there one in sight. They knew it was impossible for them, at that state of tide, to reach the shore; so, after waiting some time, and no boat making its appearance, they began to get seriously alarmed. They got on to the highest part of the stones, and began to shout for a boat to the people on shore. After some indiscriminate shouting, one of the engineers proposed that they should husband their voices, and then shout altogether, with a one, two, three. The two did so; but Dan, the staff-holder, was so overcome with panic that he kept on shouting as fast as he could take his breath, till he shouted himself quite hoarse. They were not heard on shore, for the noise made by the inrushing waters drowned their voices; but it fortunately happened that their departure in the morning had been observed, and the people on shore, seeing that there was no boat bringing them back at the proper time, got out another boat, which put off and brought them back, very late on the tide.

The situation was a very trying one to the young fellows. The engineers were no worse for it; but Dan, who was an apparently powerful young man, died from aneurism of the aorta about three years afterwards.

There is a local tradition at this passage that a corps of Cromwell's soldiers were lost here during the time of the Parliamentary wars. The men, with their officer, came to the Black Rock, on their way to Bristol, and demanded that they should be ferried across the river.

These men knew nothing about the peculiar features of this part of the river, or the nature of the ferry; and the ferrymen, who were Royalists to a man, landed them on the English Stones, assuring them that they could now *walk* the remaining distance over the rocks, which join the English shore, as they could see for themselves from the place on which they then stood.

There they accordingly left them, and took their boat back to the Black Rock, well knowing at the same time that it was too late on the tide for the soldiers to reach the shore. The Roundheads were consequently caught by the tide before they could get ashore, and every man of them drowned.

To proceed now with his narrative. After the writer had made the necessary levels, soundings, and surveys, he made the original plans of the Severn Tunnel Railway, which are, with trifling alterations to be explained further on, those on which the work has been carried out.

These plans were first deposited in Parliament in the year 1863, in opposition to Mr. Fowler's "Great Western and South Wales Direct" scheme, but fell through for lack of pecuniary support. Mr. Fowler, however, got his Bill; but the work was never carried out. The Severn Tunnel plans were again deposited a year or two later, but failed from the same cause. The writer had in the meantime, however, written full and careful reports of his Tunnel scheme, giving in detail all his reasons for knowing that it was quite practicable: he had also shown and explained his plans to Mr. George Wythes, the great contractor, who confirmed his opinion. Mr. Brassey, also, to whom Mr. Wythes had shown and explained the plans, told the G.W.R. directors that he considered it a great work, and that he should like to do it, but that he should want half a million more for contingencies in passing under the Severn.

Though the writer was unable to persuade local people to back up the scheme with their money, he had reason to know that he had convinced the G.W.R. authorities that it was practicable, and the best for their purposes. Accordingly, in 1872 he again deposited the Severn Tunnel plans, under *their* authority.

The Tunnel scheme, however, was opposed by a bridge scheme of Mr. Fowler's at the same place. This proposed to cross the half-tide rocks by spans of 100 feet, and the navigable channel of the Shoots by an opening of 700 feet, for the support of which two piers were to have been erected in the deep water of the Shoots: but the directors preferred the Tunnel; so that was the scheme adopted.

In going to Parliament with such a novel undertaking, the directors wished to have the support of an engineer of the highest standing. The writer, therefore, asked Mr. Thomas Harrison to give evidence in favour of the scheme; but, after talking it over, he could not convince him that there *might* not be somewhere in the rock forming the bottom of the Shoots such an open fissure as would make the Tunnel impracticable; he, therefore, declined to give evidence in its favour. The writer then applied to Sir John Hawkshaw, who considered the scheme feasible, and accepted the position.

After the Act had been obtained, Sir John Hawkshaw was appointed Consulting Engineer. The first object then was to ascertain by the most accurate soundings the exact form of the rock at the bottom of the Shoots for the whole distance within the limits of deviation, which had there been made 300 yards in width, in order to give the opportunity of deciding upon the best place under which to carry the Tunnel.

The taking of these soundings was a matter of some difficulty; for, in order that they should be of any real value, it was imperative that the *exact* position of each sounding should be positively fixed, and that the true levels of the bottom should at the same time be determined. As the soundings had to be taken off a boat which was floating on water having an ever-varying level, the securing of these results required some contrivance and a good deal of care in carrying them out. They also took much time, because they could only be taken at the turn of the tide, when the water was comparatively quiet. During this time the sounding went on rapidly for about twenty minutes; but, after that, the lead was carried away by the force of the current, and no bottom was felt. "No bottom" was then called, and the work ceased for the day. Thus twenty minutes only was a day's work.

The mode of operation was as follows: First, a base line had been carefully pegged out, with numbered iron bars drilled into the rock at every thirty feet along a line, square to the line of Railway, on the rocks forming the edge of the Shoots. A line of soundings was taken across the Shoots at every iron bar—that is, at every ten yards.

When, therefore, a particular line of soundings had to be taken, a pole was fixed at the proper iron bar, and a second, carrying a cross-bar near the top, at some distance behind, in a line parallel to the line of Railway. These two poles gave the exact line for the soundings. Then a third pole, carrying a large globe on top, was fixed *in the base line*, but eighty yards to the right of the first pole. All was then ready for the boat.

The sounding boat, which carried four oarsmen amidships, with two engineers at the bow and two astern, carried also a sounding machine fastened to the bow of the boat. This machine was made for the purpose; it had a reel, on which was evenly coiled a fine sounding wire, having a heavy leaden weight at the bottom end. This reel was controlled by a light hand-brake, by which the wire was kept sufficiently taut as the lead went down, and by which the reel was stopped the instant it struck the bottom. The reel also worked an index round a dial plate, which was divided into feet and tenths; so that the depth could be read off in a moment with precision. At the end of the reel there was a handle, by which the wire could be reeled up again rapidly for another sounding.

The first engineer at the bow stood over the sounding machine with a sextant in his hand, got the boat into line with the two poles, and called out "Now!" taking at the same time the angle to the lateral pole. The second engineer, with the sounding machine, immediately let down the plummet. The third engineer, with the tiller in one hand and his watch in the other, called the exact time; and the fourth noted the figures in a book ruled for the purpose—the angle first, the time second, and the depth third—as they were called out.

At the same time a man on shore was noting the heights of the water at every minute from a gauge that had been fixed there for the purpose. The soundings were afterwards reduced to the correct water levels, and the height of each sounding above Severn Tunnel datum entered in its proper column.

By these means really correct sections of the Shoots' bottom were obtained. The soundings were taken as close together as possible; and when two soundings happened to be taken on the same spot, they always tallied.

When all the lines of soundings had been taken, and the results plotted, they were found to differ so little either in the form of the bottom or in the depth of the water, that one point of crossing was practically as good as another.

When the centre line had been decided upon, very numerous additional soundings were made upon that line, so that ultimately they were hardly two feet apart all the way across; yet in no place was any fissure found: but the bottom was ridged, something like the ridge and furrow of a ploughed field, as might have been expected from the nature of the wearing away, by gravel running longitudinally up and down.

The permanent centre line of the Tunnel was now fixed and set out, and the surveys made on both sides of the river.

Where the Tunnel emerged inshore, deep and extensive cuttings had, of course, to be made on both sides of the river. These cuttings, at the gradient of 1 in 100, ran for a long distance before they got above high-water level—that on the English side, being sixty feet below the Severn alluvium, had to be carried 6,000 feet through the Severn Marsh before it got out of cutting, and involved heavy bridges and other works to carry the roads and streams across it. But it also involved another important work, as may now be explained.

From some facts he had before observed, it became plain to the writer of this paper that it would be absolutely necessary to make high sea-banks round the tunnel-mouth cuttings on both sides of the river, because the present sea-banks by the river side are not high enough to keep out very high and exceptional floods, which must occur when great storms take place upon very big spring-tides. His attention was first called to this subject in a striking manner soon after the opening of the Ferry Railway in 1863.

On the occasion alluded to, there was a great gale of wind from the South-West, accompanied by a low barometer. It occurred on a spring-tide, though not a very big one, and the tide was blown up to a height of eight inches above the highest tide he had recorded at the Pier, to which the waves also did considerable damage. This tide was blown up to a height of within eight or ten inches of the top of the marginal sea-banks of the Severn, as measured in a quiet place in Chessel Pill, where there were no waves.

Now, on referring to the tide-table, the writer observed that the tide of this day, though the biggest in that set, was still *four feet* lower than the *big* springs just fourteen days sooner or later. The question then naturally occurred to him, "What would have been the result had this same gale occurred just fourteen days sooner or later?" It would undoubtedly have risen above the marginal sea-banks, and have flooded the whole of the Severn marsh lands to a depth of seven or eight feet; and if the Severn Tunnel cuttings are never to be flooded, it will be necessary that they should have sea-banks around them of much greater height than the marginal sea-banks of the Severn.

It then became necessary to decide what this height ought to be.

The writer first tried to find out the greatest height to which *any* tides, whether spring or neap, had ever been known to have been blown up. After full inquiry, he came to the conclusion that eight feet might be taken as the extreme limit. Then, taking 3 feet 6 inches above the alluvial level as the height to which the biggest spring tide would ever rise if it came in quietly in calm weather, he obtained 11½ feet above the alluvial lands at the New Passage as the greatest possible height of the storm floods. To this he added four feet more as a margin of safety against the possible wash of the waves in such floods, and made his sea-banks round the cuttings in these meadows sixteen feet high.

In confirmation of these views, he afterwards tried to find some record of these great floods, which he was confident must have occurred in former times, though at rare intervals. The first notice of such a flood that came to him was from an old antiquarian friend – a Monmouthshire man—who had found an old book in the British Museum, called *God's Voice in Monmouthshire*, which contained an account of such a flood in the 17th century. It gave plates of the flood, showing all manner of cattle swimming in the water amongst the hedge-row trees; haystacks being floated away, and such sensational incidents; but no figures from which to learn the actual height of the flood. Thinking, then, that the churches in the low levels, of which there are many, ought to bear some record of such destructive floods, he made many inquiries, and eventually heard of two. The first of these was Peterstone, on the Welsh side, where a leaden plug had been let into the wall to mark the height to which the water rose. Peterstone was at that time a small port, with more shipping, it is said, than its neighbour Newport; but this great storm, with its great rise of tide and heavy sea, destroyed everything except the fine old church tower and one strong gable end of a warehouse, which remain to this day. The second was Kingston Seymour Church, on the English side, from a board in which has been obtained the following description of *two* floods:

On a black board, and printed in gold letters, in the vestry of Kingston Seymour Church: –

> "January 20th, 1606, and 4th year of James I.
>
> "An inundation of sea water, by overflowing and breaking down the sea-banks, happened in this parish of Kingstone Seamore, and many others adjoining, by reason whereof many persons were drowned, and much cattle and goods were lost. The water in the church was five feet high, and the greatest part lay on the ground about ten days.
>
> (*Signed*) "WILLIAM BOWER."

On another black board, in the porch of the same church, is written:—

> "On November 27th, 1702, a flood occurred, destroying the sea-bank, drowning much cattle, and destroying hayricks, etc."

It is observable here that the *biggest* tides never occur either in January or in November.

These great storms on a very big spring-tide are so rare that they hardly occur perhaps once in a century, and people generally do not believe in a thing that has not happened in their lifetime, or in that of their fathers; hence the notion of these great sea-banks to keep out Severn floods has been somewhat ridiculed. The writer may mention here a singular case which occurred later on. In 1883, on the occasion of a visit of some of the directors to the Severn Tunnel works in the middle of September, the chairman laughingly remarked that he thought these high banks could hardly be necessary, for the South Wales line had now been opened thirty-five years and the rails had never yet been flooded, the sea-bank there being nine or ten feet above the rails. The writer's reply was that he was quite confident they would be wanted, and that, at any rate, they cost nothing, for they were made by barrow work, and the saving in the cost of the earthwork more than paid for the additional land. And there the matter dropped; but on the 18th October following a heavy S.S.W. gale occurred upon a big spring-tide, and blew the water to such a height that it flowed over the commissioners' sea-bank and flooded the marsh lands to such a depth as to run over the South Wales line of rails eighteen inches deep, and at the same time to flood the Marsh shaft, and pour down it in a great cascade. There were fifty men below at work; they all ran to the cage to be hoisted up, but the water had put out all the engine fires. Three or four of them then went to an upright ladder against the shaft wall and all but one succeeded in getting up, in spite of the water falling upon them: but the other fell off and was killed. The rest of the men below went back to the higher workings, and climbed up on the centering. The water poured in until it had risen to within three feet of the crown of the arch. This occurred at eight o'clock at night. The contractor's engineers above were trying to devise some means of getting the men out, but could do nothing to rescue them until early the next morning, when they got a boat from the pier. This they let down the shaft *end on*—for the shaft was not wide enough to take it when level—and then they had much trouble to float it on the water. They had to make it dive first, and afterwards to bale it out. There was just room to paddle it under the crown of the arch, and in this way they got to the men about noon, after they had been there about sixteen hours in the dark. This was a narrow escape, for a very little more Severn water would have quite filled up the length of tunnel then completed.

These sea-banks round the tunnel mouth cutting are therefore clearly necessary in order to prevent the tunnel from being flooded at some future day—a dreadful catastrophe if it ever did occur; and they were provided for in the original setting out of the line upon the ground.

After the soundings had been completed, and the centre line and works set out, the time had come to start upon the actual work.

The directors had wisely decided that the *first* work undertaken should be the heading under the Shoots. In this way the nature of the ground and the full practicability of the whole scheme could be actually proved before the great outlay upon the remaining works was incurred.

The intention at that time was to sink a shaft upon the

SEVERN TUNNEL.

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low-water rocks, close to the edge of the Shoots, and from thence to drive the heading.

For this purpose, a strong tower would have had first to be built upon those rocks to a height well above the highest tides; and then, working in that, the shaft could have been sunk. But there was so strong an opposition to the building of this tower at that place on the part of the navigation, that the idea was at length abandoned, and the shaft was placed on shore at Sudbrook. The two other shafts, which were to have been placed on the English Stones, were also given up. This opposition seemed somewhat captious on the part of the navigation authorities, who might have had to put up with the piers of a bridge all across the river, and who had already been beaten in their opposition to the bridge scheme of Mr. Fowler, a few miles higher up. The objections of the pilots were something fanciful, as may be imagined from their objection to the building of the beacon on the Charstone Rock, by the South Wales Union Co., some years before, for the safety of their ferry passage, which beacon has *now* become the most important landmark for their navigation of the Shoots channel, and concerning which, if it were now removed, there would no doubt be the most dreadful outcry. One must suppose that they object to anything *new*, whether good or bad.

The Sudbrook Shaft was started on shore in March, 1873. It began in new red rock and marl, in which a good deal of water was found, and then passed through a 4-feet bed of magnesian conglomerate. Down to this, 91 feet from the surface, the strata were horizontal. The shaft then went through Pennant sandstone, 19 feet; clay shale, 24 feet; coal shale, 10 feet; millstone grit, 11 feet; fireclay, 9 feet; mountain limestone lumps in fireclay, 15 feet; and firestone, 26 feet; all these dipped towards the river 1 in 7.

The river heading was begun in December, 1874, and passed again through the dipping beds into the Pennant. The bed of coal shale was very heavy ground, which had to be double timbered and floored, which made the heading small and low at that point. The Pennant was at first of a red colour, and full of *backs*, or open joints, from which flowed a good deal of water. This water at first was of the colour of weak porter, and had a very salt and bitter taste; but after a few days it ran clear. What was tapped first had no doubt been lying there for ages undisturbed, the colour showing probably the presence of iron. Some copious springs were afterwards found in the Pennant; so a flood door was built in the Pennant, 340 yards from the shaft, to prevent the flooding of the pumps in the event of a breakdown, which it did on two or three occasions. This door was very strong, and large enough to let the trolleys pass through; it also had a 12-inch sluice valve.

And now, seeing that this heading would have to be driven *two miles* under the river from this one shaft, it became of great importance to adopt the most correct means possible of getting the lines of the Tunnel down the shaft and along the heading. The usual way has been to drop a plumbob down each side of the shaft; but this was found to be not nearly accurate enough for the purpose.

The plumb lines could only be got thirteen feet apart; and though the bobs were large, and dropped into buckets of water to steady them, they could not be made *really* steady, for a pendulum two hundred feet long vibrates slowly. It has a beat of nearly eight seconds each way; and when the extent of the vibration is less than an eighth of an inch, it *appears* to the eye to be steady; but when the cross hairs of a transit below are fixed on the wires, first one and then the other appears to leave the line, and you may, as the writer did, spend an hour in the vain endeavour to fix them, until your handkerchief is wet through with mopping up your tears, but you are never sure of accuracy. In addition to this, there was a considerable *jar* in the bottom at every stroke of the big pump, which shook the wires and kept them in a nearly constant tremor. So this plan was abandoned and a new one adopted. A good transit instrument was obtained for the purpose from Messrs. Troughton & Simms, with a four-inch hole in the bottom plate, through which the telescope could be pointed vertically downwards, and a large block of stone was built solidly into the shaft wall on top to carry the transit; the heavy end of the stone was built into the wall, but a lighter end projected two feet into the shaft. On this lighter end the centre line of the tunnel had been marked, and an eight-inch peep hole cut through it. This formed the apparatus for the shaft top. For the bottom a pianoforte wire, about two hundred and fifty feet long, was procured, and a couple of fine-threaded screws, made each about six inches long, and mounted on a bed plate which could be firmly screwed down to a beam of timber. Then a beam was strongly fixed across the back of the shaft bottom furthest from the heading, which was then only being driven towards the river, and another beam across the heading within a foot or two of the full length of the wire; the adjusting screws were fixed upon these beams in their proper places, for the position of the centre-line was known very nearly at the shaft bottom, and pretty nearly at the two hundred and fifty feet along the heading, which was then perhaps two hundred yards in. The wire was then carefully uncoiled off its reel, so as to leave no kinks, and each end was passed simply *over* the thread of the screw at each end with a 28 lb. weight suspended from it. This kept the wire clear of the heading floor all along. The screws were well oiled; they worked by a simple hand-wheel at one end. All was now ready below. The new transit was fixed over the hole in the top stone, and adjusted in true position and level. It was then turned upon a flagstaff on the other side of the Severn and clamped there, being now in the true centre line of the tunnel.

When this had all been accurately done, the telescope was pointed down the shaft, where the screw and the wire passing over it could be plainly seen. A few turns of the screw brought the wire into exact position. This being fixed, the telescope was then directed to the furthest point of the wire that could be seen before it entered the heading, and that point was then brought into true line by means of the screw in the heading two hundred and fifty feet away. This, of course, took a little more time, because it had to be done by signalling.

This having been accomplished, the readings were a *second* time observed, to make sure there was no error of observation. Electric lamps had to be used below in order to get a distinct view of the wire, and then the cross-hairs of the transit could be made to bisect the wire with great accuracy.

The wire now gave two hundred and fifty feet of base line very correctly set out. The engineer below, with a smaller transit, then fixed his instrument in the line, and about ten yards further in than the end of the wire, and adjusted it until he could exactly cut the wire correctly at both its ends; he was then in a position to fix correct marks in the roof-trees all along the heading so far as he could see.

These lines were checked throughout by the same process several times afterwards; and when the heading became very long, the large transit was taken below to continue the lines more correctly under the Severn. After they had been run through a first time and the points marked, the telescope was turned over in its Y's, and the whole line set out a second time to see if there was any instrumental error. It was then found that there *was* a slight instrumental error, for the two lines gradually diverged in a very flat curve, and were nearly two feet apart at the end of a mile—the *true* centre line was consequently just half-way between the two, and thus the permanent centres were fixed.

The heading was then carried forward under the Shoots, where the Pennant was of a grey colour, and so hard and close that no joints were visible, until the Shoots had been passed. The heading then began to rise again, according to the gradient, and after a time emerged from the top of the Pennant into the upper coal shales, where it passed through two beds of coal, ten and twelve inches thick, until it came to a vertical fault, 2,180 yards from the shaft, where it suddenly entered the new red sandstone. This rock was very hard to drill (machine drills were used, driven by compressed air), and when blasted, the dynamite only blew out a round hole; but blew the sandstone into *powder*. This powder was of a singular nature: after it had been filled into the iron trolleys, in passing along the heading it got shaken down so tightly that all the water was driven to the surface, and the sandy material below was wedged in so fast that when the trolley was run to the tip it would not come out, but carried the trolley with it, rolling over and over down to the bottom, where it was found tight in the trolley still; so that the men had afterwards to set the trolley up on end, and get the stuff out with a pickaxe. The writer never met with any material like this before, except in "Kellaways Sand," in Wiltshire. If you there dug a hole in the ground, the stuff that came out would not fill the hole again. A man of his there had to fix a big gate-post, and after the post was in he had not stuff enough to fill the hole again, but had to fetch another barrow-full; the stuff afterwards set almost like mortar.

After the heading had passed through fifty yards of this material, it came into an open vertical joint, two feet wide, which was full of water under pressure. When the first drill that went into it was withdrawn, the water spurted out with such force as to knock down the ganger flat upon his back, as he was standing about four or five yards from the face, looking on. The men said it flew fifty yards along the heading before t struck the ground, and caused great alarm amongst them at the time; but the pumps took it all, and in a day or two the quantity of water had dwindled down to a moderate sized spring. This wide fissure is traceable on the English Stones overhead, but it is quite filled up there.

After the heading had been driven under the whole breadth of the Shoots, and had thus demonstrated the extremely sound character of the rocks there, the other shafts were sunk, and headings were started in the line of the Tunnel from all of them. A heading was at the same time begun *landwards* from the Sudbrook Shaft; and it was in this heading, when it had attained a length of 354 yards from the shaft, that the big spring was first tapped, and flooded the works on the 16th October, 1879. The heading under the river had then reached a length of 3,370 yards, or nearly two miles from Sudbrook Shaft, and was within 138 yards of joining the heading from the other side.

It may be mentioned here that when the headings met some two years or so afterwards, the Engineer-in-charge reported them as meeting "dead true both in lines and levels."

After the works had been flooded, the directors made Sir

John Hawkshaw the Engineer-in-chief, and let the completion of the whole work to Mr. Thomas Walker, the very able and energetic contractor, who has since completed the undertaking.

At the time, and in order to master the big spring, new shafts were sunk on both sides of the spring—two at Sudbrook, and two about half a mile further in-shore. In these powerful pumps were fixed, and when they were got to work, the head of water was gradually lowered until it was only about forty feet deep at the bottom of the shaft. In addition to the water from the big spring, it was known that a large quantity came from the river heading and through the flood door that has been mentioned. In order to get rid of this for the moment, it was of much importance to shut this door, if possible. The contractor therefore engaged a noted diver to make the attempt. His name is Lambert, a fair-haired man of few words, but of great courage. Lambert made a very plucky attempt to get to the door in his ordinary diving dress: but his air pipe floated so hard against the rough roof of the heading that he could not drag it along quite far enough to get to the door. He therefore was compelled to turn back when within seventy yards of the door: and it was in retracing his steps that he had dreadful trouble to get his air pipe back again, and through the narrow, close-timbered part through the coal shale; for it curled up in kinks and coils about the beams and timbers of the heading. Everything had to be done by himself in entire solitude, and by groping about in perfect darkness; he had to find and gather up, and to carry forward these numerous folds of his air pipe, some of which were constantly slipping from his grasp; and all the while he knew that his life depended on his management of this pipe. It was astonishing that his courage could have sustained him through such a trial; for the writer's belief is that if ever his heart had failed him, he would not have come out alive.

He was dreadfully disappointed at his failure; but the contractor, having heard of the new apparatus for diving *without air pipes*, which Fleuss, its inventor, was exhibiting daily at the London Aquarium, invited him to come down with his patent apparatus and close the door.

Fleuss accordingly came there, and went down, with Lambert, to the mouth of the heading; but when he had groped about and found the sort of place he was expected to go into for nearly a quarter of a mile, his heart failed him, and he came up again. He said he would not undertake to go and close the door for ten thousand pounds. Lambert then asked him to lend *him* the dress, and *he* would go; but this Fleuss refused to do.

When, however, the contractor pointed out to him that he was standing in his own light, for if Lambert accomplished the job by the help of *his* patent dress it would be the best advertisement possible for him, Fleuss did consent; so the next day Lambert put on the dress, and closed the door after one failure, which was caused by the mask he had to wear pinching his nose so tightly that it brought on a terrible headache, which made him turn back to have it altered. In this apparatus the breath has to be taken in through the nose and breathed out through the mouth, therefore the nose has to fit very tightly to the nose pipe; and as Lambert's nose was very broad, while Fleuss' was thin, the nose pipe pinched him dreadfully, and he had to get it altered.

The water was all out, and the work started again by the end of the year 1881, after having been flooded fourteen months.

Sir John Hawkshaw had, in the meantime, recommended that the level of the rails should be depressed fifteen feet under the Shoots and along the English section of the Tunnel, where the original gradient of 1 in 100 should be maintained, but that on the Welsh side it should be made 1 in 90. The directors consented to this, and an Act was obtained accordingly.

After having got out the water, the contractor pushed on the works with great energy. Amongst other works, he had to drive a new heading from Sudbrook Shaft towards the big spring at the 15-feet lower level. While driving this, and when still ninety yards from the old spot, the big spring broke in again in doubled quantity, and in such force that the miners and their half-loaded trolley were all washed out into the shaft together. A door had been built there, but no one could get near it, for the water poured out at the rate of 27,000 gallons a minute. Large pumping power was available at that time; so the head of water was gradually lowered until the heading was bared, and the water from the spring ran down it nearly breast deep.

Three of the most powerful of the miners then tried to shut the door by creeping up against the side of the heading, the two hinder men shoving the leader up against the current. He got up to the door-frame; but the moment he put out his arm to grasp the door, the three of them were whirled round and washed out into the shaft.

Lambert was again brought there. The head of water was allowed to rise in the shaft until it had sufficiently subdued the flow of water to allow him to go up against the current and close the door. He said that when the door slammed the concussion was terrific, and made him see all the stars of heaven inside his helmet.

The contractor had also some other troublesome works to do. The new drainage culvert from the pumping shafts to the Shoots, at the 15-feet lower level, was very troublesome as well as the lowering of all the shafts and pumps; but the worst job he had to do was in making the Tunnel on the landward side of the big spring. The conglomerate bed, which constantly varied in thickness from four feet at Sudbrook shaft to 26 feet at 5 miles 4 shaft, was encountered along half a mile of Tunnel from the big spring westwards. It was very full of water, and very hard to excavate. In some places the water poured in in such quantities that it was almost impossible to keep any lights burning, shade them how you would. The execution of this part was very costly. It was by far the wettest part of the Tunnel, and proved eventually to be that in which the *water pressure* did the most damage to the brickwork, as described presently.

The brickwork of the Tunnel throughout its whole length has been built in the form shown in the drawing. The form is a very good one, approaching as nearly as circumstances would admit to a cylindrical shape. The greater part was put in through very hard, rocky strata, and common bricks *only* were used in its construction.

Where the invert rests upon a rock bottom, the small piece of level footing below the side walls should be omitted. Also, if common bricks alone are used, there is a weak point in the three-cornered springing block, between the invert and the bottom of the side walls, where the brickwork has to sustain the greatest pressure of all, for a good part of it can only be filled up with broken bricks and spawls; and as a good brindle brick will bear at least twice the crushing weight of the best cement, this three-cornered piece is necessarily weak. The proper remedy for this is to have some *special* bricks made to suit the angle. One form of special brick alone is necessary, by the use of which, together with common bricks, as shown on the drawing, this three-cornered piece, which is really the springing of both the side walls and the invert, may be built as strongly as any part of the work.

And now as to the effect of water-pressure—

The river Severn is naturally the lowest drainage level of the district. In any works going *below* that level, the water met with must either be pumped out or forcibly kept out under pressure.

In a land tunnel the water runs out of itself at one end or the other, and is of no further trouble after the work has been completed, the brickwork has only to support the weight of the ground overhead; but in the Severn Tunnel he *water-pressure* is the main consideration.

In discussing this question, it may be as well first to say a word as to *Land* Tunnels.

If the Tunnel should be in rock or self-supporting strata, no lining is needed for the actual support of the ground; but it has been found better to put in a thin lining of brickwork in railway tunnels even in this case, for in blasting the rock, some pieces are so far loosened that though they do not come away at once, they do fall subsequently at an uncertain date after the tunnel has been opened for traffic, and thus become a source of great danger to the trains. No invert is needed in this case, and sometimes no side walls; but in regard to side walls, the cost of dressing the rock on the sides, and of forming a good springing for the arch, make the saving so small that it is usually better to build side walls as well as an arch.

In soft or yielding ground an invert is also wanted with heavier arch and side walls; but it is a fact well known to practical engineers that the greatest weight on the brickwork is always met with in *shallow* tunnels; that is, when they are only sixty feet or so below the surface; the ground then breaks up to the top, and the whole superincumbent load comes upon the arch. But in deep tunnels, the ground does not break up very far above the arch, and therefore it mainly supports itself.

The chief precaution to be adopted in order to insure permanent stability in a tunnel arch and side walls is to have the space at *the back*, between the excavated ground and the brickwork, quite built up with bricks and mortar. There must always be a space left over the arch where the timbers are withdrawn, even if the ground has been got out with the greatest truth; but, in addition to this, miners have usually a tendency to take out too much ground at the haunches of an arch, for some hidden reason which appears to be common to mining judgment, and if this space is not *completely* filled up, the effect can readily be imagined. An arch, it must be remembered, is a *balanced* structure and of great strength when properly loaded; but in the case now supposed, it will be found that as the ground by-and-by naturally comes down heavily where it is least supported—that is, over the crown of the arch—if the space above the haunches has not been quite filled up, the crown of the arch sinks under the load and the haunches rise until they *do* get a bearing against the ground above, and the arch is badly crippled, more especially if the brickwork has been built in *rings*, for the inner rings then gape at the crown, and some of the bricks often fall out. When land tunnels fail, it is almost always because this back filling has been neglected; and this can only be seen to during the progress of the work, and not afterwards.

In the Severn Tunnel, on the other hand, the chief load on the brickwork is from the *water* pressure, which differs entirely from the land pressure, as described above; in that the pressure is from all directions, and precisely according to the head of water at each point. Referring again to the sketch of this Tunnel, the *water* pressures on the various parts of the work are figured in lbs. on the square inch at every two feet in height. It will be observed that while the pressure on the crown of the arch is 52 lbs. on the square inch, that under the invert is $64\frac{1}{2}$ lbs.

These are the direct water pressures which have to be carried by the brickwork acting as an *arch*, which has to bear this converging pressure all round, and to convert the vertical load into a thrust or *bed-pressure* on the brickwork all the way round the Tunnel.

Now these bed-pressures are many times greater than the water pressures outside, so they are figured in cwts. per square inch, and they vary directly in proportion to the radius of curvature of the arch. The result, therefore, is that while the water pressure on the crown of the arch is 52 lbs., the bed pressure on the arch is 3·15 cwts. on the square inch; but the invert, being of a flatter radius, while the water pressure was $64\frac{1}{2}$ lbs., the bed pressure is 6·09 cwts., or just about double what it was on the arch. So, to make them of equal strength against water pressure, the invert should be twice as thick as the arch.

In fact, however, the 27 inch invert would have been quite strong enough to have borne the thrust if the load had been fairly distributed through the thickness of the work; but as it was, it did give way in some places in the wet part of the Tunnel before alluded to.

The cause of the failure of the work to bear the great water pressure may probably be explained in this way: In putting in the invert on a rocky bottom full of water springs, after the rock has been got out, the bottom is of course rough and broken, and the water wells up out of every tiny crevice or crack, as well as out of the wider joints in the rock, and this water cannot all be got rid of by drains or by pipes to let it through the brickwork. It can readily be understood then that in building in the successive rings, that put in first against the rock would be the roughest and most pervious to the water, the next put in would be better, and the third better still; the conditions under which each successive ring is put in being constantly more favourable the further it is removed from the water. The natural result of this would be that the rings, as a rule, were least leaky towards the top of the invert, and most leaky towards the rock bottom.

In the case alluded to, after the work had been put in two or three months, when the cement was supposed to be set, the pipes were corked. The water pressure came on the brickwork and had reached a pressure of about 60 lbs., when in certain places the *top* ring of the invert was blown up. After the top ring was gone the *second* was blown, and after that the *third* was rising, when the contractor opened the pipes again and took off the water pressure.

The explanation appears to be simple. The top ring in these places was the tightest or least leaky of all the six rings put in, and therefore the water which came with comparative ease through the lower five rings was checked by the top ring, and nearly the whole of the water pressure came upon that one ring, which was only 4½ inches thick—a pressure possibly of nearly two tons on the square inch—and this was more than that one ring could bear; the cement joints yielded each a little (for no crushed brick was found), and the ring was blown up. The same process was repeated at the second ring, and afterwards the third was following suit when the pressure was taken off by uncorking the pipes.

This only occurred in certain parts of the invert; in the other places, no doubt one of the deeper rings might have been the least leaky, and this ring having the support of those above it was able to bear the pressure.

As may be supposed, there were places in the arch and side wall which also flushed off under the pressure. After full consideration it was finally decided to pump *all* the water, including that from the big spring, and in consequence of this decision, a new shaft to contain six very large pumps had to be sunk at Sudbrook; and the opening of the railway had to be delayed a year or more in order to get this, with the necessary machinery, boilers, and buildings, all erected.

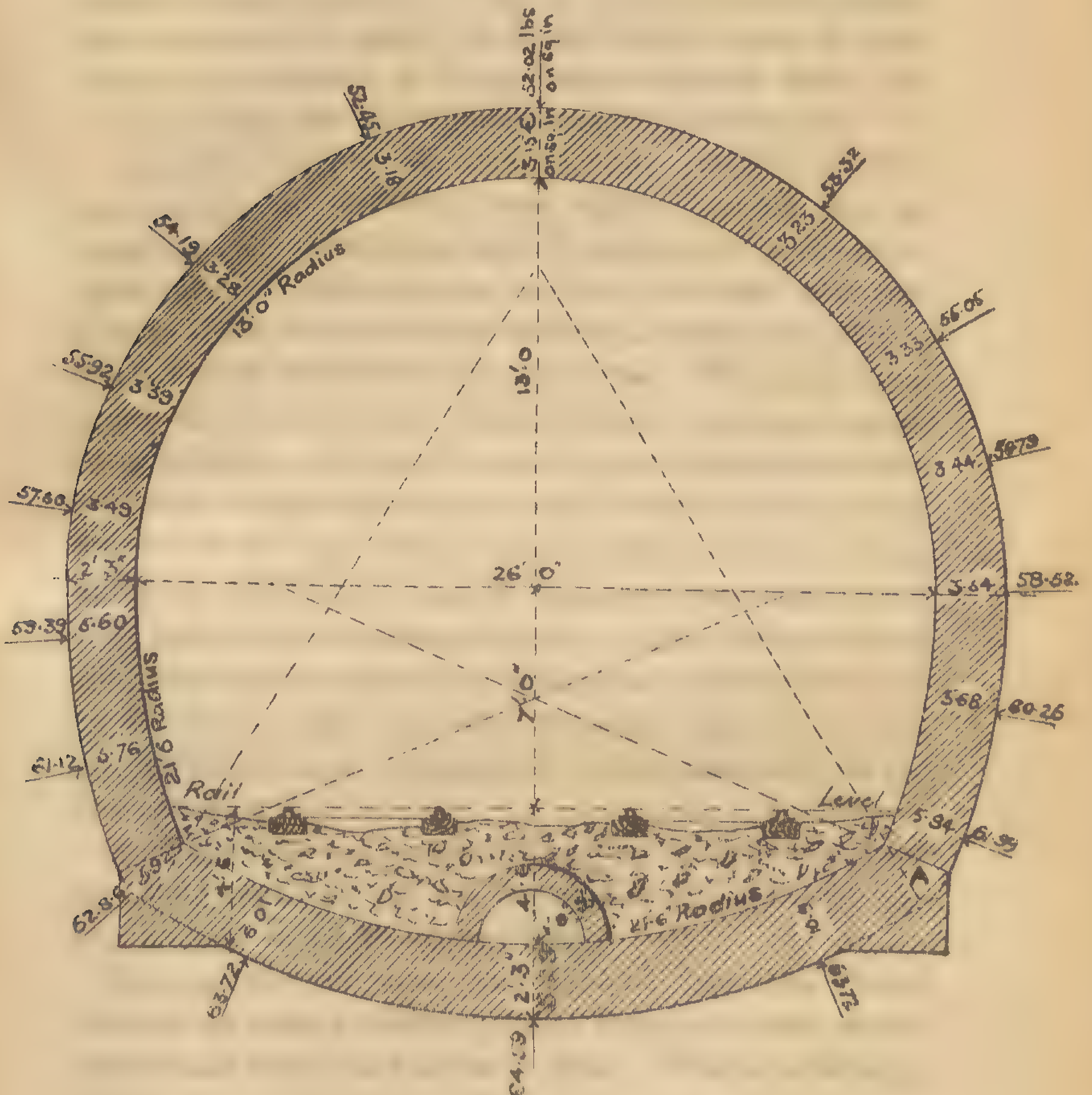
It may here be mentioned that the writer advocated the vertical bond instead of ring-work in the arches; and, in consideration of his views on the subject, a special provision was made in the specification to the contract that this bond should be used if ordered by the engineer-in-chief. He, however, objected to the use of *special* bricks, which would have been necessary to make the bonded work, and so none were used. It is impossible to say what amount of *leakage* might have taken place through the bonded work, had the Tunnel been so constructed; but it could not have been *blown* like the ring-work. The writer should add that he did not foresee that in ring-work under great water-pressure the rings would be blown one after the other, as they actually were; for he had never seen such a thing. He merely knew that an arch so built was immensely stronger in every way than a ring arch—much as a well-bonded house wall is stronger than one would be if built entirely of stretchers, like so many 4½ inch walls placed one against the other without being bonded together.

The Severn water did get into the Tunnel in two places near the English shore. The new red strata, which are so hard under the central parts of the river, are there much softer; so that at low water the level of the river bed is eight feet lower than on the English Stones proper, and a shallow low-water basin is left by the tide, some three or four feet deep in the deepest places, and called the Salmon Pool. On the other hand, the Tunnel gradient, rising at the rate of 1 in 100 from the Shoots, comes nearer to the bed of the river there than at any other point. When the heading was enlarged into the full-sized Tunnel, the Severn water in its muddy state did find its way through into the Tunnel below, and for a time flooded the sea wall portion of the works. On examining the river bottom at low water, a large hole was found, and the contractor then cut two wide gutters to drain the Salmon Pool, and afterwards filled the hole on top with bags of clay. This enabled him to build the arch of the Tunnel under that ground, and to make all secure The same thing occurred also a few chains further on, and was got by in the same manner; but the clay in the bags gradually ran through the pipes with the water, and the holes had eventually to be filled with gravel and coarse sand, which stopped the leakage from the Severn. The *spring* water from below, however, continues to flow into the Tunnel in many places under the Severn, and is of a strongly mineralised character, some of the springs colouring the walls of the Tunnel a bright red, and others a jet black. One of these black waters has a strongly sulphureous smell. They are also, as a rule, saline to the taste.

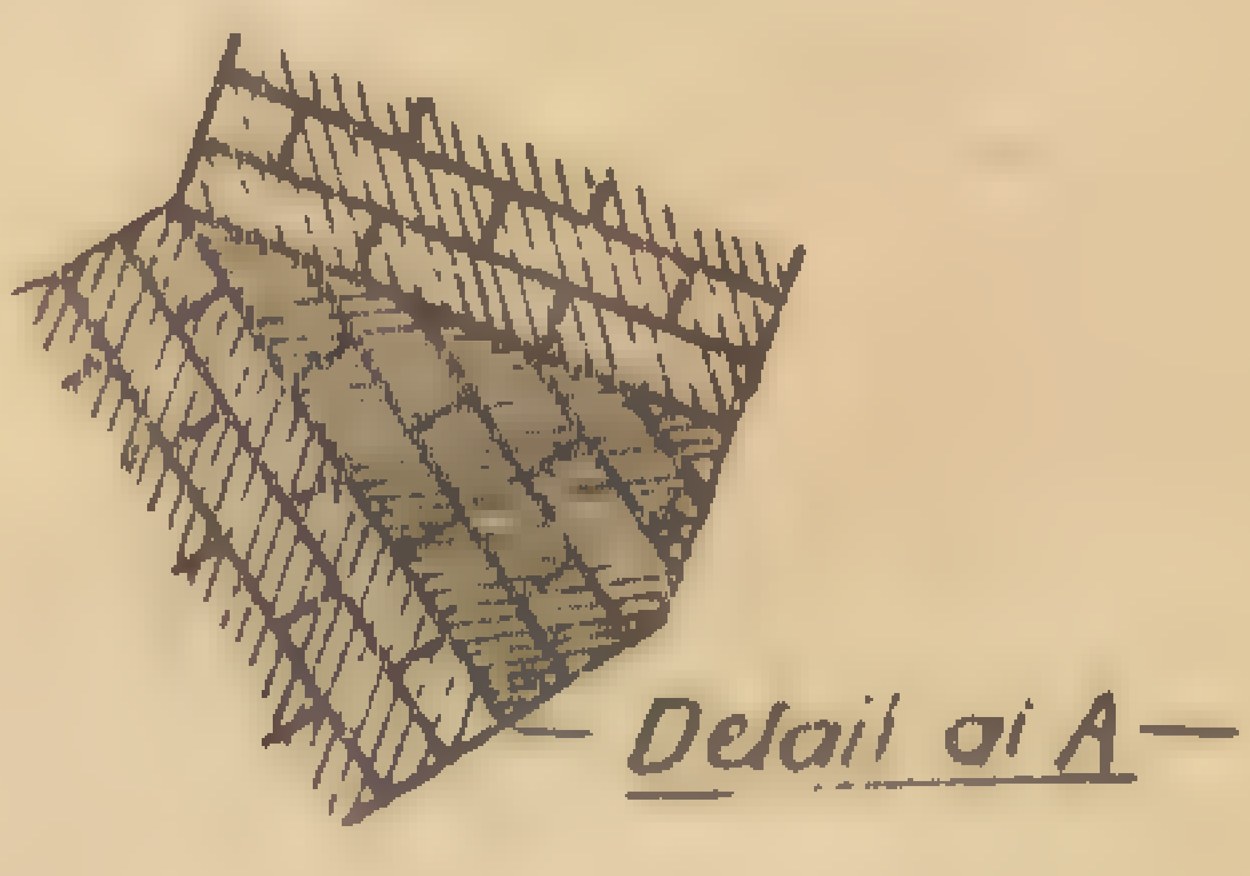
In making the great cutting on the English side through the Severn marsh lands, some curious discoveries were made. These lands are all level and unbroken on the surface, and were evidently formed at some period by deposit from the Severn. The lowest stratum, lying on the new red, was coarse gravel, with large masses of old red conglomerate, and of mountain limestone, deposited here

Plate XVI.

SEVERN TUNNEL SECTION.



—Water Pressures in lbs per sq. in.—
—Bed Pressures in cwts per sq. in—


—Detail at A—

—Scale—

and there; then a finer gravel, and on this a clear river sand. All this was quite free from mud, having been deposited in water moving too fast to let the fine mud remain. Then came silt or muddy sand, and then a layer of peat, which is remarkable in being below half-tide level, and therefore many feet below the water level of every tide.

After this came a pale-coloured clay, and then another bed of peat, two feet thick, upon which was a blue clay up to the top soil. Both these beds of peat are below the high-water level of ordinary tides, and yet it is said they were of fresh water origin, and the trunk of a large oak tree was found buried below the upper peat bed. The beds above described preserved their level—though sometimes undulating slightly—all along the cutting, and must have been deposited a long time ago, for there were found four river courses, as the men called them (creeks or pills, as they would be called here), cutting through these beds, which must have indicated a former course of the Chessel Pill, or some such tidal watercourse, for they were filled with more recent Severn mud; and this must have taken place long ago, for there was no indication of their position left on the surface.

The works were all completed, and the Tunnel was opened for goods traffic on the 1st September, and for local passenger traffic on 1st December, 1886.

The quantity of water now pumped from the Tunnel amounts to twenty million gallons a day, of which eleven millions are from the Sudbrook spring. This is after many weeks of unusually dry weather.

The Severn Tunnel Section.

By Prof. C. LLOYD MORGAN, F.G.S.

IN the section across the river Severn which accompanies Mr. Charles Richardson's paper, there are several features of geological interest. I propose very briefly to draw attention to some of these under four heads: (1) The Alluvial Beds, (2) the Trias, (3) the Palæozoic Rocks, (4) the River Channel.

1. *The Alluvial Beds.*—Mr. W. C. Lucy has kindly given me permission to make use of the following note:—

"On the visit of the Cotteswold Club to the Severn Tunnel, in May, 1885, I made a very hurried inspection of the Drift. In coming from South Wales, after leaving the tunnel on the Bristol side, the cuttings are full of interest. The upper surface consists of about ten feet of Yellow Clay, one or two feet of Peat, six feet of Blue Clay, one foot of Peat, five feet of loamy Sand, and then a mass of Gravel, twelve to eighteen feet thick, resting upon New Red Marl. There is evidence of the existence of at least four pills" (marked "mud" on the section), "cutting through the alluvium, occurring at different levels. The bottom of the largest is six feet under present low water level in the channel. *Cardium edule* and *Tellina solidula* were found in the bed of sand in the gravel. The bottom bed of the gravel is very coarse compared with the upper beds, and a short distance before the thinning out of the ground, it is more angular, and little water-worn, clearly showing that it came east of the river. There are large boulders of quartz, millstone grit, coal-measure sandstone, slate (probably from Cumberland), Lickey pebbles, fine quartzites, Old Red conglomerate, trap rocks, and rolled Lias gryphites. I found one piece of glaciated diorite. The Drift is probably of two ages. One may be considered as belonging to the low-level drifts of Mr. Prestwich, in which is mixed up high-level drift with rocks derived from far distant places."

The four pill-courses mentioned by Mr. Lucy are very clearly shown in the section. They were without doubt excavated in the blue marsh clay, and underlying beds subsequent to their deposition, but are older than the yellow surface clay which overlies them. Their symmetrical shape would seem to me to indicate that they were not long occupied as watercourses.

In the Caldecot shaft (2 on the section), which was sunk through the alluvium of the river Nedden, a band of *Scrobicularia* marl was found, 39 feet from the surface. "It is of yellowish grey colour, and is characterised by an admixture of fresh-water and brackish-water shells, such as *Limnæa*, *Planorbis*, *Scrobicularia piperata*, and *Cardium edule.* Diatoms are common in it, and also remains of *Chara.*" (*W. J. Sollas.*)

Prof. Sollas classifies the alluvial deposits in the following manner:—

Zone 1.	*α* Upper Clay.	*a.* More sandy zone, five to seven feet. *b.* More argillaceous zone, with disseminated vegetable matter.
	β Upper Peat.	

Zone 2. α Lower Clay.
β Lower Peat.

Zone 3. α Sands and Mud.
β Gravel.

Concerning these deposits, Prof. Sollas says:—

"The presence of glaciated stones in the gravel is very suggestive. . . . The sand is evidently a marine or tidal deposit, like the blue clay, from which it differs chiefly in containing no argillaceous sediment, or scarcely any. . . . The lower peat must evidently have accumulated at a time when the relative level of land and sea was different from that of the present day. At New Passage, for instance, it lies at half-tide level, and would have been, under existing conditions, always submerged for one-half the day. An elevation of twenty feet, however, is all that is required to bring the layer of peat up to the same level as that of the existing surface of the ground; and more than this cannot, I think, well be allowed, for the tidal deposits lying conformably above and below both beds of peat seem to point to a simple movement of depression, interrupted by occasional pauses. The tidal mud beneath the peat proves that during its formation the margin of the land was below water, just as surely as the layer of peat proves that during its formation it was above. We can account for the silt on the theory that it was a case of 'deposition during depression;' and we can also account for the peat by supposing that the downward movement ceased for an interval, during which deposition continued and led to the silting up of the creeks and bays of the estuary to above the high-water level of ordinary spring tides; extraordinary spring tides would raise the level somewhat above 'high-water spring' mark, and a marsh-growth would raise it somewhat higher still. . . . After a long pause, during which the lower peat, with its associated remains of forests, was formed, subsidence set in again, and the lower blue clay gradually accumulated over the layer of vegetable matter. Another interval of rest succeeded, during which a new alluvial surface rose above the tide, and the upper bed of peat was produced; again the movement of depression was renewed, and apparently not uniformly, since the lower part of the upper clay contains scattered fragments of plants which were probably derived from some exposed portion of the peat, which the waves ploughed to pieces and distributed far and wide. . . . Finally, the upper part of the blue clay was deposited, and this is more arenaceous than the lower part of the same deposit, because the continued depression had brought the sea nearer the area of deposition." (*Q. J. G. S. XXXIX.* 623–4.)

Of the blue clay of the Severn alluvium, Professor Sollas says: "The blue silt of the Severn alluvium is strikingly similar in all its characters to the modern ooze; it consists of a similar admixture of mud and angular silicious fragments, while marine Sponge-spicules, Foraminifera, Coccoliths, and other marine remains similar to those of the modern silt, are universally disseminated throughout its mass." (*Loc. cit.* 615.)

The peat in the tunnel section, as at Porlock and elsewhere, contains abundant remains of yellow iris flags. Tree trunks (oak) were found, but I am not aware of any discovery of mammalian remains.

In this connection a quotation from Mr. Lucy's paper, in the *Proc. of the Cotteswold Club*, on "The Submerged Forest, Holly Hazle, Sharpness," may be of interest:—

"The Peat Bed, near Sharpness, lies in a hollow, very much resembling a trough, excavated out of the lower beds of the Old Red Marl. . . . It attains a maximum thickness of nearly fourteen feet, and, as it rises near the surface, thins out to a few inches. It is composed mainly of oak, alder, beech, and hazel, and towards the top of it some nuts of the latter were found quite perfect. Some of the oaks were of considerable size, indeed one was quite a giant of the forest, measuring, as it lay, 80 feet, and 2 feet 9 inches in diameter at the top. The base was a good deal decayed, and, as far as could be ascertained, exceeded 5 feet in diameter. . . . Where the peat was the thickest, and within about two feet of the bottom of it, were found a fine head of *Cervus elephus*, antlers and jaw bones of rather a small deer, head of horse, and *Bos longifrons*, skull of a dog, and tusk of the boar. On showing Prof. Church, of Cirencester, the head of the *Cervus elephus*, he pointed out to me what had escaped my observation, that the antlers had been cut off, apparently by some rude implement." (*Loc. cit.* 1872, p. 112–13.)

2. *The Trias.*—These beds call for but few remarks. Overlying the Palæozoic beds is a band of Dolomitic Conglomerate of variable thickness,—twenty-eight feet in Shaft No. 3; four feet in the Sudbrook Shaft, No. 4. It consists of limestone fragments embedded in a very hard reddish matrix. The fragments vary from half an inch to two feet or more in diameter. Some of them have peculiar striæ, described by Prof. Sollas in the *Geol. Mag.* for February, 1882.

Above the Dolomitic Conglomerate lies the Keuper Marl, 65 feet in thickness, containing, as at Aust Cliff, beds of gypsum. This is succeeded by yellow and white Sandstone, 13 feet thick. Mr. Charles Richardson informs me that the white Sandstone was close and hard, and an excellent building stone.

We know from the Aust Cliff section that the Keuper beds are there 114 feet thick. This is all we have in the Severn Valley to represent the 3,500 feet of Keuper in Cheshire. The Bunter, more than 1,000 feet thick in Cheshire, is wanting. The Permian beds are also wholly absent. In this district, therefore, after the upheaval of the Palæozoic formations, there followed a prolonged period of continental conditions and consequent denudation. During this period many of the sculptured features of Severn scenery were produced. These were subsequently buried beneath mesozoic strata, and it has been the work of post-mesozoic denudation to remove the secondary wrappings around this ancient sculptured surface.

3. *Palæozoic Rocks.*—During the construction of the Bristol and South Wales Union Railway, the great Cattybrook fault was discovered. This brings Coal Measures against Upper Limestone Shales. Farther down the river, at Portishead, a great fault brings Pennant against Old Red Sandstone. Such faults, which may have had much to do with the determination of the line of the Severn Valley, prepared geologists for the existence of strata of Coal Measure age beneath the Severn. The section shows how these beds have been proved in the Severn Tunnel. It also shows the occurrence of minor faults.

Concerning the Palæozoic rocks, Mr. Evan D. Jones, sometime assistant engineer to the Severn Tunnel works, has contributed valuable information to the *Proceedings of the Geologists' Association*, vol. vii. From him I quote:—

"The Coal Measures were entered under the Triassic Conglomerate, and the information on this point afforded by the excavations for the Severn Tunnel, is that to which the greatest interest attaches. The Pennant Grit, passed through under the Conglomerate in this shaft, had a vertical thickness of 19 feet, dipping towards the centre of the river, or a few degrees S. of E., at about 1 to 12, its upper surface here forming the plain of denudation upon which the Trias has been deposited. Beneath the Grit were 35 feet of the Lower Coal Measures, dipping in the same direction, 25 feet consisting of Clay Shales, and 10 feet of Coal Shales, separated by a 2-inch seam of coal. These rested on Millstone Grit, 11 feet thick, under which were intermingled beds of Firestone and Shale, about 6 feet thick Under the Shale came a remarkable bed made up of lumps of Mountain Limestone, with angular corners, embedded in the Shale, 14 feet in thickness; the lumps varied in size from small pieces to blocks as large as a wheelbarrow. The remaining 26 feet of the shaft penetrated a very hard, close-grained, red firestone, or ironstone (as it was called by the miners), occurring in regular beds from 6 to 12 inches thick, separated by layers of hard fire-clay about an inch in thickness.

"From this shaft two headings have been driven under the river, a lower one commencing at the bottom of the shaft, and an upper one 40 feet from the bottom, the former being continuous, and nearly two miles long.

"The upper heading begins in the Millstone Grit, and the floor of the heading continues in it for 50 yards, but the roof passes into the Coal Shales at a distance of 10 yards. The length of Coal Shales passed through was 20 yards, and it was followed by the Shales for a length of 110 yards, when the Pennant was entered.

"In the lower heading the same kind of rock that occurred at the bottom of the shaft was found for a distance of 40 yards, when the limestone blocks in shale were reached.

The limestone was here continued for 90 yards, occurring first in strips about three feet thick (of which there were six interlaminated with shales), but becoming more massive towards the end, where a block about 12 yards thick at the bottom and tapering away to a point at the top was met with, which appeared to be but a projection of the limestone underneath into the overlying shales.

"The next 40 yards were in the bed of shale which was found under the Millstone Grit in the shaft, and were followed by 40 yards of Millstone Grit, 30 yards of Coal Shales, and 70 yards of Clay Shales, the two latter representing the Lower Coal Measures. Resting upon these, the Pennant was again entered. It extended in one solid mass (with the exception of a few beds of interlaminated clay shales) for nearly a mile, carrying the heading about 500 yards beyond the Shoots channel. A considerable quantity of water was met with in various places in driving through it, about 150,000 gallons an hour altogether. The Upper Coal Measures were next entered, and were found to consist mainly of hard Clay Shales. In the portion immediately overlying the Pennant two seams of good hard coal were struck, one 15 inches and the other 10 inches in thickness. Some thin beds of Pennant were also found in this shale. The heading continued in these Upper Coal Measures for about 400 yards, when a sudden change took place, and the New Red Sandstone, which is here faulted down against the Coal Measures, was entered.

"Appended is a tabular list of the strata passed through in Shafts No. 3 and 4, and the heading from the latter driven last, which are the most important parts of the work geologically considered :—

Shaft No. 3.

		Feet.
	Fine Sand and Gravel	18
Trias	Yellow Sandstone	12
	Red Marl (very hard)	68
	Dolomitic Conglomerate	28
Carboniferous	Pennant Grit	4
	Lower Coal Measures (Clay and Coal Shales mixed)	20
		150

Shaft No. 4.

		Feet.
	Fine Sand and Gravel	7
Trias	Yellow Sandstone.	13
	Red Marl (very hard)	65
	Dolomitic Conglomerate	4
Carboniferous	Pennant Grit	19
	Lower Coal Measures { Clay Shales, 25 feet Coal Shales, 10 ,, }	35
	Millstone Grit	11
	Shales (very hard)	6
	Limestone and Shales	14
	Red Sandstone (hard and close grained)	26
		200

Bottom Heading (from Shaft No. 4.)

		Yards
Carboniferous	Red Sandstone (same as in bottom of shafts	40
	Limestone and Shales	90
	Shales	40
	Millstone Grit	40
	Lower Coal Measures { Coal Shales, 30 yards Clay Shales, 70 ,, }	100
	Pennant Grit	1,600
	Upper Coal Measures (broken by Fault)	400
Trias		all beyond the Fault.

"The first feature to be noticed in connection with these excavations is the great variety of strata which have been met with, and their perfect geological sequence. This is shown in a striking manner in Shaft No. 4. This shaft is only 200 feet deep; nevertheless, in sinking through that distance, the New Red Sandstone, Dolomitic Conglomerate, Pennant Grit, Lower Coal Measures, and the Millstone Grit occurred within a vertical depth of 154 feet.

"The vertical thickness of the Pennant Grit here is 19 feet, as stated, its dip averaging 4·75°, and its horizontal length 1,600 yards. Calculating from these data, its actual thickness may be stated as 400 feet, or less than a fourth of its average thickness in the Bristol Coal Field; but the greatest anomaly is in the thickness of the Lower Coal Measures and the Millstone Grit, which here have an actual thickness of only 35 feet and 11 feet respectively, as against 2,000 and 1,000 feet in the Bristol Coal Field.

"In the cutting near Portskewet station on the South Wales Railway, nearly half a mile to the north-west of the shaft, the Mountain Limestone is exposed, coming very nearly to the surface, dipping E.S.E., or nearly in the direction of the shaft, at an angle of about 5°.* From this it would appear that the Limestone ought to be found at the shaft at a depth of about 200 feet from the surface. Very nearly at this depth the peculiar lumps of Limestone in Shale, and the interlaminated beds of Limestone and Shale were found, which show that in all probability we are there on the top of the Mountain Limestone.

"It may be mentioned with regard to the bed of Millstone Grit, that the distinctive character of the rock would leave no doubt as to its identity, even if this could not be established independently by its position.

---

* The average dip of the Carboniferous beds, ascertained by observations taken in the shafts and headings, and by comparisons of the relative positions of the beds in the shaft with those in the headings, is about 4·75° corresponding to a fall of 1 in 12.

"From the distinctive character of the rocks, together with the regular sequence in which they occur, and the proximity of the outcrop of the Mountain Limestone in the vicinity, we are led to believe that the Lower Coal Measures and Millstone Grit thin out in this district to a mere strip, notwithstanding that they attain greater thickness in the Bristol Coal Field.

"We know precisely where the bottom of the Pennant is; we have every reason to believe that 50 feet lower down we are on the top of the Mountain Limestone, so that the Lower Coal Measures and Millstone Grit must be wholly contained in the intervening space.

"The mixed beds of Shale, Limestone, and Sandstone which occur at the bottom of Shaft No. 4, and probably also for a short distance lower down, might be added to the Millstone Grit, thus increasing the thickness of the series to that extent; but as thin beds of Millstone Grit are usually found among the top beds of the Upper Limestone Shales, I have included this portion of the shaft in the Limestone series.

"As far as can be ascertained, the actual thickness of the strata on the western side of the river Severn are as follows:—

Trias	85 feet.
Upper Coal Measures	unknown.
Pennant Grit	400 feet.
Lower Coal Measures.	35 ,,
Millstone Grit	11 ,,

The bed described by Mr. Jones as "close-grained red firestone" is that marked "Mountain Limestone" (blue) on the section. Mr. Charles Richardson informs me that if effervesced slightly with acid. The bed of Shale containing lumps of Mountain Limestone is puzzling. From the description Mr. Jones gives, I should be disposed to believe that the limestone was *originally interstratified with the shale.* The section shows how much the beds are broken and disarranged near this point. One would be glad to know whether any fossils were found in this limestone. Mr. Richardson does not remember having seen any. Remembering that Fort-Major Austin described beds of Limestone as occurring in the Millstone Grit, one cannot but feel some doubt as to whether the true Mountain Limestone, or even the Upper Limestone Shales, were reached. The Millstone Grit in the Forest of Dean is nearly 500 feet thick. Near Bristol it is nearly twice that thickness. The thinning out of the Lower Coal Measures is remarkable. Here again, one cannot but regret that the broken character of some of the beds should make one feel that any interpretation of the sequence here may possibly be somewhat "at fault."

4. *The River Channel.*—There can be little or no doubt that Mr. Charles Richardson is correct in ascribing the "Shoots" channel to the wear and tear of the river bed, due to special denudation along this line. Mr. Stoddart's "ancient fissure" theory is practically out of court. There is one remark in Mr. Richardson's paper, however, to which I should like to draw attention. He says, "The Shoots Channel, as you ascend the river, points towards the Welsh shore, so that the *in-flowing* tide presses against the Welsh shore; while, on the other hand, the main stream of the *ebbing* tide is flung towards the English shore by the projecting point of St. Tecla's Chapel Rock, thus leaving an intermediate sandbank, called the Dun Sand, in the middle of the river, between the up and down currents. The ebbing tide, thus thrown on to the English side, passes over the

English Stones so long as the tide is high; but as half-tide is approached, the waters, having no longer an outlet that way, rush along the head of the English Stones down to the Shoots." Now I have very little doubt that along the line of the English Lake and English Lake Channel (see map), a new channel for the ebbing tide is even now in progress of formation, and that in the near (geological) future, all the ebbing tide will be carried off by an English Channel, while all the incoming tide will, as now, pass through the Shoots.

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ENGINEERING SECTION.

BRISTOL NATURALISTS' SOCIETY.

"*Rerum cognoscere causas.*"—VIRGIL.

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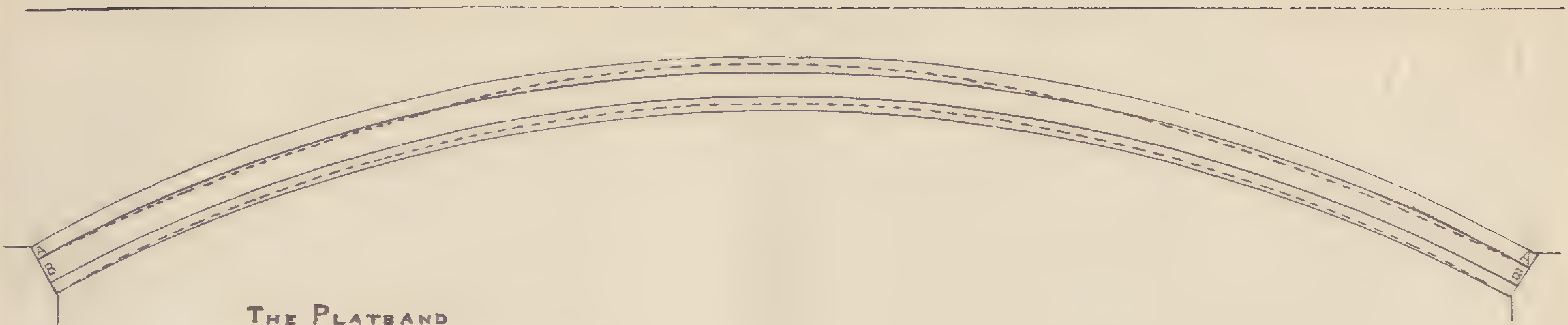
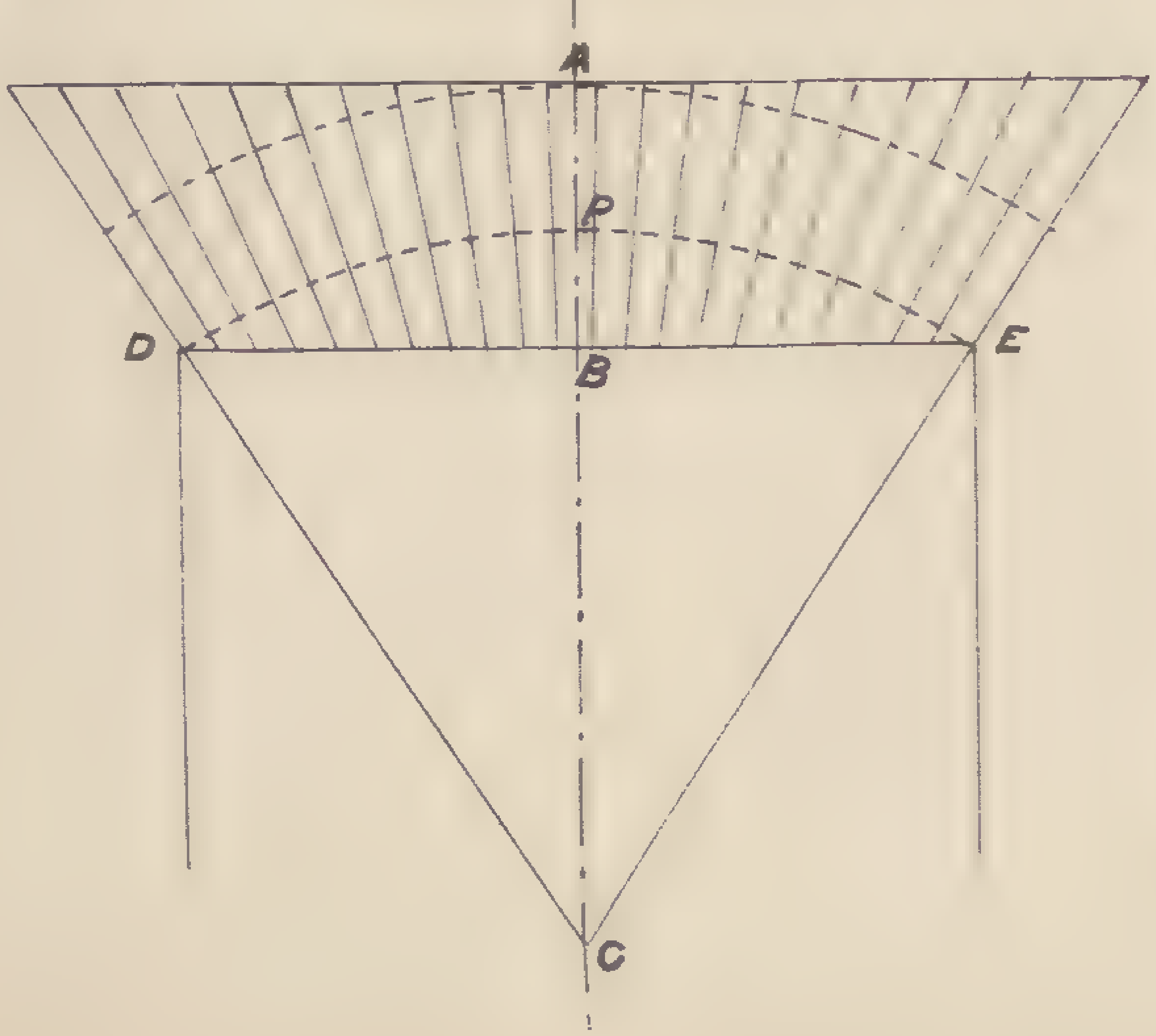
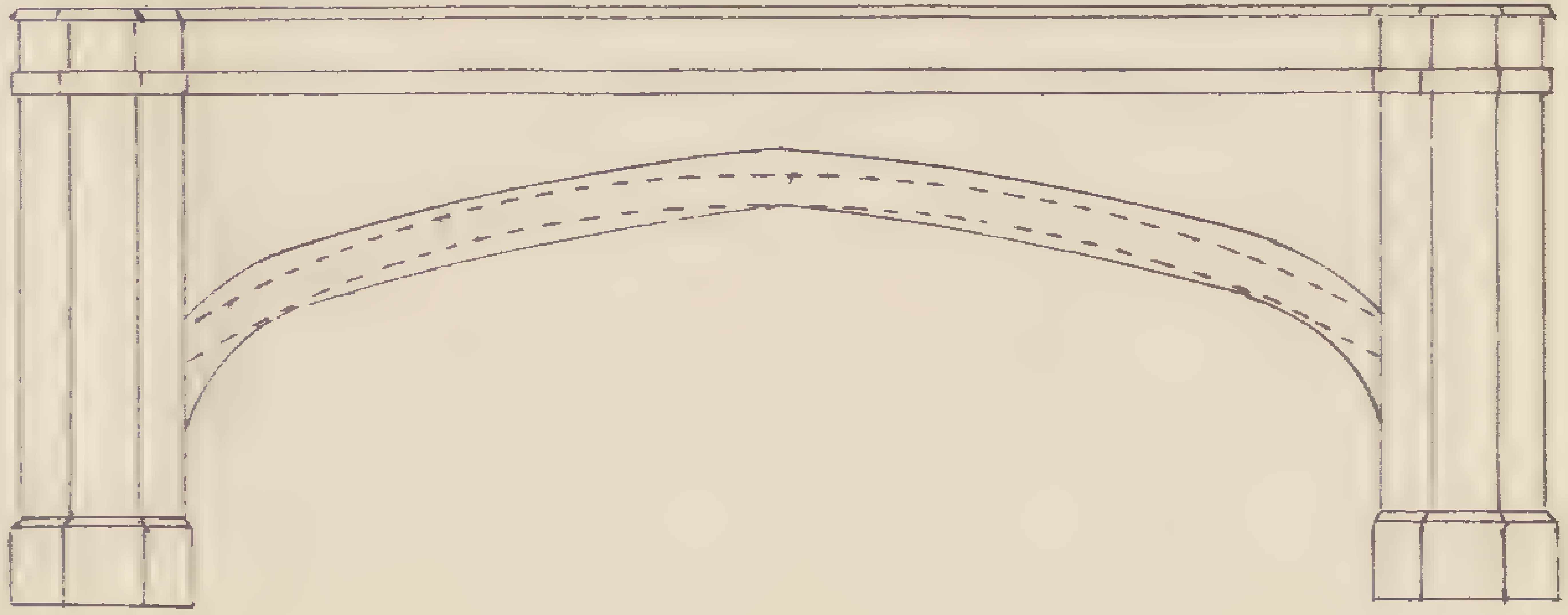


THE PLATBAND



FLAT GOTHIC ARCH



The Arch.

By CHARLES RICHARDSON.

A paper read before the Engineering Section of the Bristol Naturalists' Society, on the 18th of October, 1887.

WHEN speaking of arches, Dr. Hutton says: "A large and elegant bridge, forming a way over a broad and rapid river, is justly esteemed one of the noblest structures that the skill of man can raise."

That arches are naturally picturesque structures we may infer from the fact that all artists bring a *bridge* into their landscape whenever they can; we may therefore understand that the Doctor's assertion is fully borne out when the bridge is both "large and *elegant*," but an arch cannot be elegant that is not in accord with the true balance of natural forces.

Arches have been built for many centuries; but the calculation needed to ascertain their true form, with a full knowledge of the pressures and thrusts at every point, involves the higher mathematics, which were only first discovered in the time of Newton. Before that, therefore, the building of arches must have been a mere question of trial and error, and subsequently of *taste* combined with precedent. For instance, in our old Gothic architecture we have in the best examples a marvellous display of great taste and boldness of construction, where the cultivated eye may observe that the arch thrusts are all admirably accounted for, and that the intermediate groinings are carried by slender and lofty columns, all beautifully proportioned to the pressures or loads they have to bear.

Doubtless this amount of skill was only arrived at gradually, and through numerous failures in the first instance, or by what has been called trial and error; but the exquisite *taste* of the old architects has been displayed in bringing these noble structures to the perfection we now witness and admire.

In every structure, we may take it that suitability to its object and surroundings is the chief element of stability as well as of beauty. As we indeed find it throughout Nature: everything there is most suitable to its purpose, and everything also most beautiful to the eye.

Thus, in arches, a lofty arch of comparatively small span is most suitable in a lofty building like a church, where top weight has chiefly to be borne, as it is also the most beautiful; but under a flat roadway an arch of wide span and small rise is by far the most beautiful as well as the most fit for its purpose. The arch should be of the true curvature due to its natural equipoise, as is explained further on, and as is also exemplified in the roadway bridge by its suspended counterpart—a suspension bridge—where the natural and graceful curve of the chains always forms its chief beauty. Lastly for a tunnel, the semi-circular curve is the fittest and looks the best, because it has to support heavy pressure all round.

The Gothic architects of old, without doubt, displayed the greatest taste in their structures, in providing against the disturbing effect of the horizontal thrust of their lofty arches by the addition of ordinary and flying buttresses to countervail this thrust, and in making these buttresses a great ornament to their structures; they appear in fact, to have had the taste to make all the parts necessary to the utility or stability of their buildings contributory to their beauty also—a grand proof of their exquisite taste; but they, as has been said, can have known nothing of the true theory of arch-building, and must therefore in that respect have acted from precedent alone.

Now, on the contrary, we have the means of calculating exactly all the thrusts and pressures as well as the true form of the balanced arch under all required conditions; so that our *experimental* knowledge is confined simply to that of the weight and strength of the materials which have to be relied upon in the construction of any arch.

We may now proceed, in the first place, to consider the principle involved in the construction of an arch.

An arch, at all times, is a *balanced* structure which, when correctly built, maintains itself between two, more or less distant, fixed points of support, or abutments, in such manner that all its parts shall be in perfect equilibrium whatever the weight of the arch itself or that of the superstructure placed upon it may be, and however that load may be situated.

Structurally, the theory of the arch is this: Referring to the upper diagram (page 103), which represents the simplest form of arch—namely, a rude structure of only three stones between the abutments. This rude arch, however, involves the whole theory and principle of the balanced or "equilibrated" arch, as it is called.

Let us suppose the two abutments to be, say, 6 feet apart, and that the three stones are roughly squared flagstones which, placed end to end on a floor, will cover a length of say, $7\frac{1}{2}$ feet; it may be conceived that if two of these stones be placed against the abutments and the third in the middle between them, as shown in the sketch, it *might* be possible so to place them that they would balance each other and remain as a rude self-supporting arch, notwithstanding the so placing of them by hand would be a work of the greatest difficulty.

Now, though we may be unable to do this by hand, yet Nature will solve the problem for us at once in this way: Let us only imagine that the stones are strong magnets, and that the three stones and the abutments are turned upside down, so that the three stones shall be in *suspension* from the abutments, perfectly free to move at the joints, but held closely together by our supposed magnetic attraction; *then* the three stones would naturally fall at once into their true positions, and if we could only replace them in these exact relative positions when again turned up, we should have a perfectly balanced structure.

This principle of inversion and suspension is true of all balanced arch structures, whether they are of the simplest form, as in this case, or in any more complicated form, and whether they are arches of construction or inverted arches of suspension; both are dependent on the same vertical force of gravitation combined with horizontal thrusts. All that is necessary is only that the structure should be *balanced.* The difference between the two forms being, that the arch has to be *constructed* in truly balanced form, while the inverted arch *falls of itself* into its perfectly balanced position. The one is equipoised by art, the other by Nature.

If, for example, we take a string of beads, and hook up the two ends of the string, we know that the beads will range themselves immediately into the form of an inverted arch, notwithstanding that the weight, form, and size of each bead may vary to any extent. Now we know this only from experience and the belief that natural laws are constant. But on the other hand, if we do know the relative forms, sizes, and weights of every bead, we might find it beyond our powers of calculation to define *beforehand* the precise form of the inverted arch that these beads would at once assume when suspended, unless indeed the problem were simplified by making all the beads equal or varying only in certain symmetrical ratios. Yet, in all cases, if we were able to preserve the exact relative positions and bearings of these beads forming the inverted arch, when we turned it up they would always form a truly equilibrated arch which would support itself perfectly on its two extremities. The powers of Nature so far excel the powers of art.

Returning again to our simple diagram (No. 1), we are supposed to have here an arch of three stones in perfect equilibrium; this means that the counterbalancing forces applied to the angles B and C in opposite directions should exactly balance one another--consequently, if the opposing forces are known, this may be proved by means of the well-known mechanical law of the "parallelogram of forces."

But in order to apply this law to our diagram with greater clearness, let us suppose that the three stones are represented by three stiff rods, AB, BC, and CD, abutting against the fixed points A and D, and let us further suppose that these three rods are of the same weight as the stones which they each represent, and that they are in their truly balanced position, though perfectly free to move in the vertical plane.

It is evident, in the first place, that these balanced rods are only kept in position by the naturally opposing thrusts at each angle being exactly equalized; that is, that the rod A B leans or thrusts against B C with a force which B C resists with *perfect* accuracy, because they are balanced; for if the thrust of either were in the *slightest degree* altered, the equilibrium would be destroyed and the arch would tumble down at once.

In this diagram B *e* and C *f* are supposed to be vertical lines, and the other lines C *e* parallel to A B, *e g* to B C, B *f* to C D, and *f h* to B C.

Now, in the parallelogram *g* B C *e* we have a parallelogram of forces. If the lengths of the lines *g* B and B C represent the opposing thrusts against the point B in those directions, then B *e* must represent the *vertical* load on B (or suspended from B, which is the same thing). Also at the point C, B C and C *h* represent the opposing thrusts in those directions, and C *f* the vertical load on C; and it is evident to the eye, from the comparative acuteness of the angle C, that there is a greater vertical load on C than on B.

Let us draw the horizontal lines B *k* and *h l* perpendicular to C *f*, then it is evident, with regard to the thrust B C against the angle B, it is made up of the horizontal thrust B *k* combined with the downward vertical thrust C *k*. Again, the thrust of C *h* against *h* (or its continuation to D) is composed of the horizontal thrust *l h* and the vertical C *l*; but B *k* is equal to *l h*, because the two triangles are equal; therefore B *k* and C *l*, which represent the horizontal thrust, are the same in both cases. In like manner, if from the points C and *g*, perpendiculars were drawn to the line B *e* produced, the horizontal thrust there would be also equal to B *k*. We see, therefore, that the horizontal thrust is the same in all three sections of the arch, notwithstanding the ever-varying *vertical* loads; this principle is *true* of *all* arches whatever their span—the horizontal thrust is always the same in all parts of the arch.

Again, if the diagram is turned upside down, it will then represent an inverted or suspended arch, and it will be found that the same law of the parallelogram of forces will equally, and in the same manner, prove all the problems above mentioned, the only difference being that the thrusts become tensions and the tensions thrusts; but the proportional *amounts* of these forces, represented by the various lines, remain exactly the same as before, in the direction of those lines: the horizontal tension here also remaining constant throughout the length of the arch, while the vertical load continually varies.

This proves the truth of what has been before stated, that if an arch be truly equilibrated in construction, it will be equally true in suspension, and that *every* constructed arch may be considered to have its suspended counterpart, in which the forces in every part are precisely the same both in energy and in direction, and that reasonings based upon the one will always and equally apply to the other.

Having now learnt what we have from Diagram 1, which represents the simplest possible form of arch, let us proceed to learn what we can from Diagram 2.

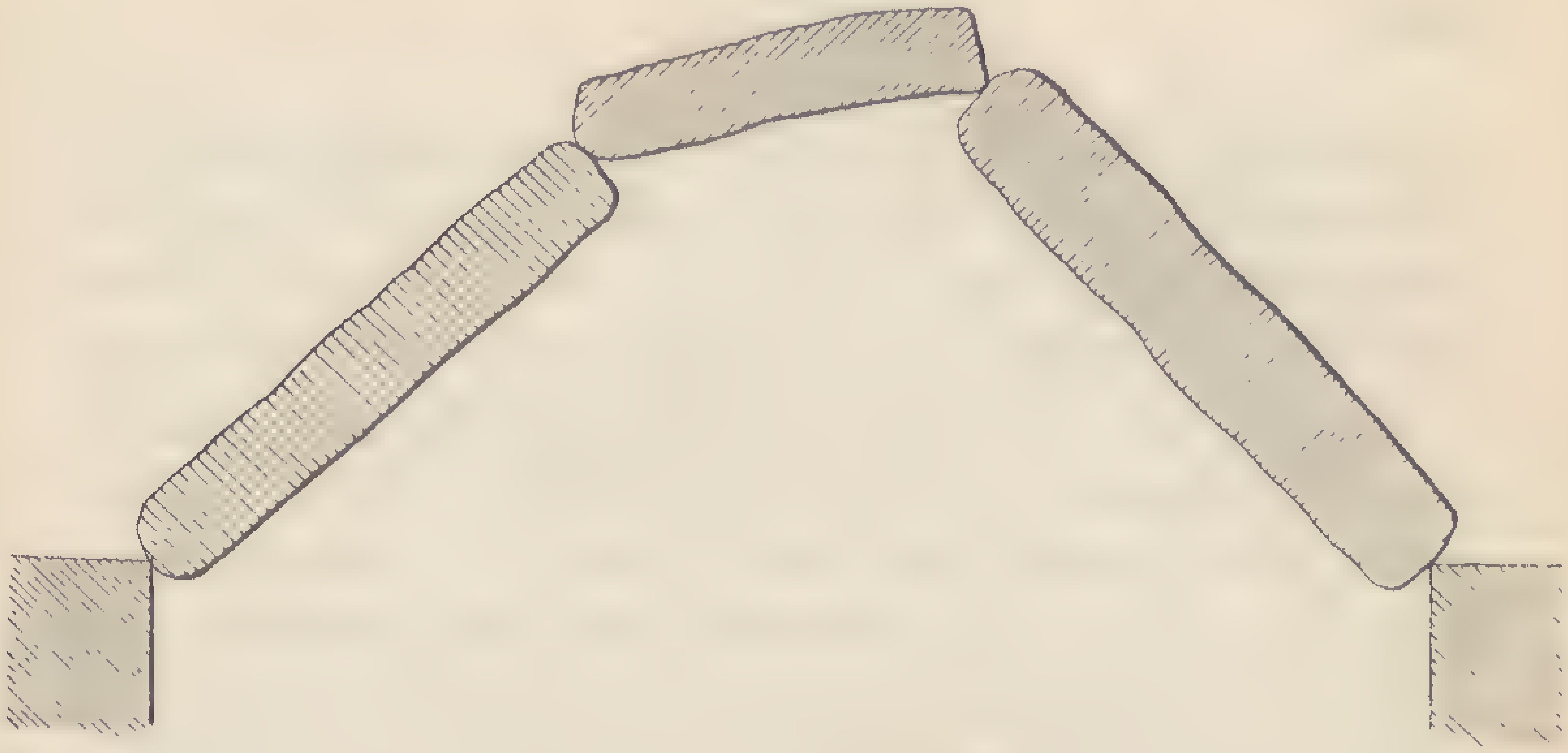
We may here suppose that we have an equilibrated arch of six rods or stones, A B, B C, C D, D E, E F, and F G, between the abutments A and G, supporting a horizontal roadway T R; that these rods meet at the angles B, C, D, E, and F, of which D is the central, and is also the middle point in the arch; and that A T, B S, etc., are vertical lines from the respective angles up to that road line, D O being the central of these lines, and its length termed the "crown thickness" of the bridge. These lines must of course represent the vertical loads on each angle, if all the material between the soffit of the arch and the road line is throughout of the same specific gravity.

Now it will be recollected that in the case of Diagram 1, in order to find all the thrusts acting upon the three rods, we drew the parallelograms B h and C g, and proved that the thrusts along A B, B C, and C D were represented respectively by the lines g B, B C, and C h, that the vertical loads on the angles B and C were represented by the verticals B e and C f, and that the horizontal thrust in every case was represented by B k.

Following the same rule of construction in the present case, but without drawing the entire parallelograms, which are not now necessary and would entirely confuse the figure; let us begin at the central point D. First draw a vertical line $z m$ through D, and C i parallel to D E, meeting $z m$ in i; we now have a triangle of forces, C D i, in which, as proved in the former case, each side will represent the thrust in its own direction—C i the thrust along D E, D C that in its own direction, and D i the vertical load on the angle D, which vertical load is also represented by O D, therefore D i is equal to D O, and, further, the horizontal line C h drawn from C to the line $z m$ will represent the horizontal thrust throughout the arch. To proceed now to the next angle E, C i is already drawn parallel to D E, let us draw C k parallel to E F, meeting $z m$ in k; then it follows, as before, that C k will represent the thrust along E F, and $i k$ (equal to P E) the vertical load on E. In the same manner draw C l parallel to F G and C m parallel to the dotted line through G, then C L and C M will represent the thrusts along F G and the dotted line through G respectively, and $k l$ and $l m$ (equal to Q F and R G) the vertical loads on F and G.

On the other side draw C x, C y, and C z parallel respect-

ARCH DIAGRAMS



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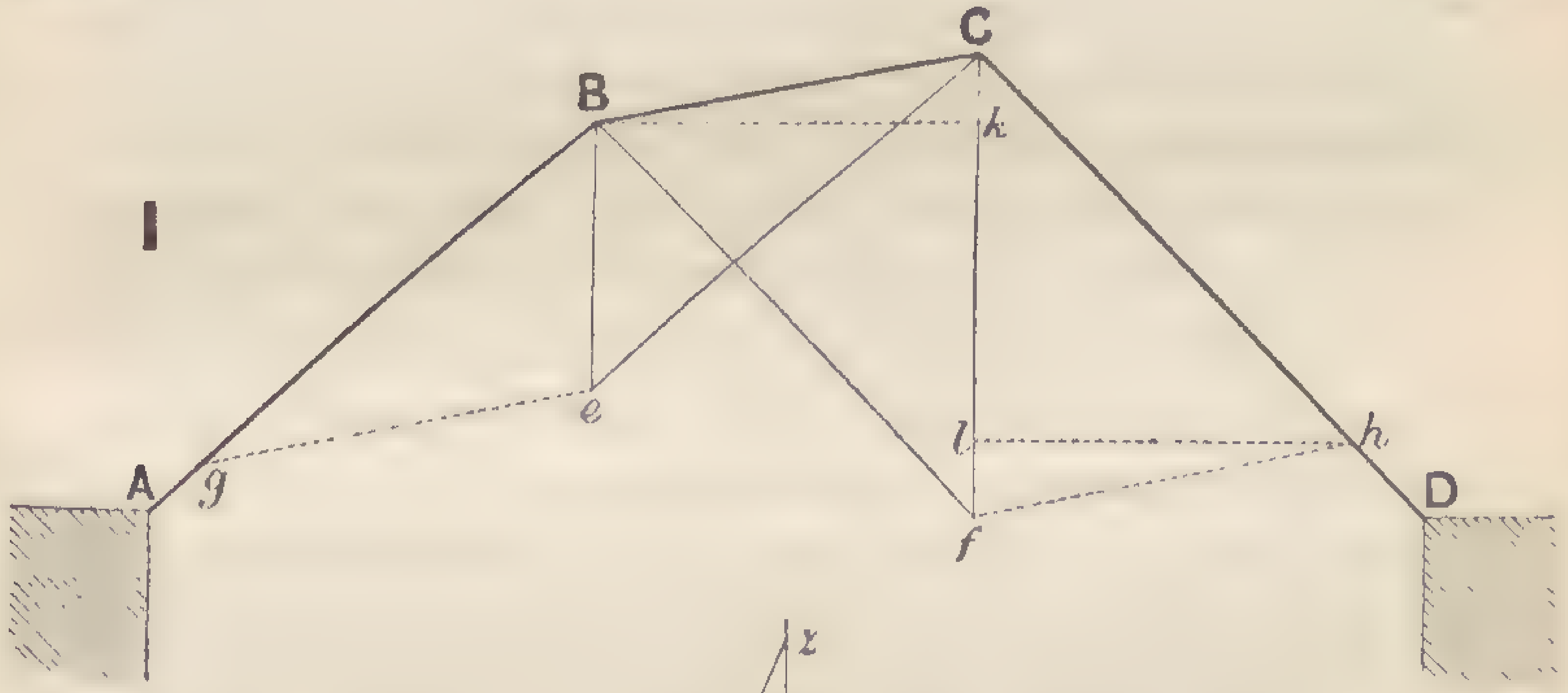

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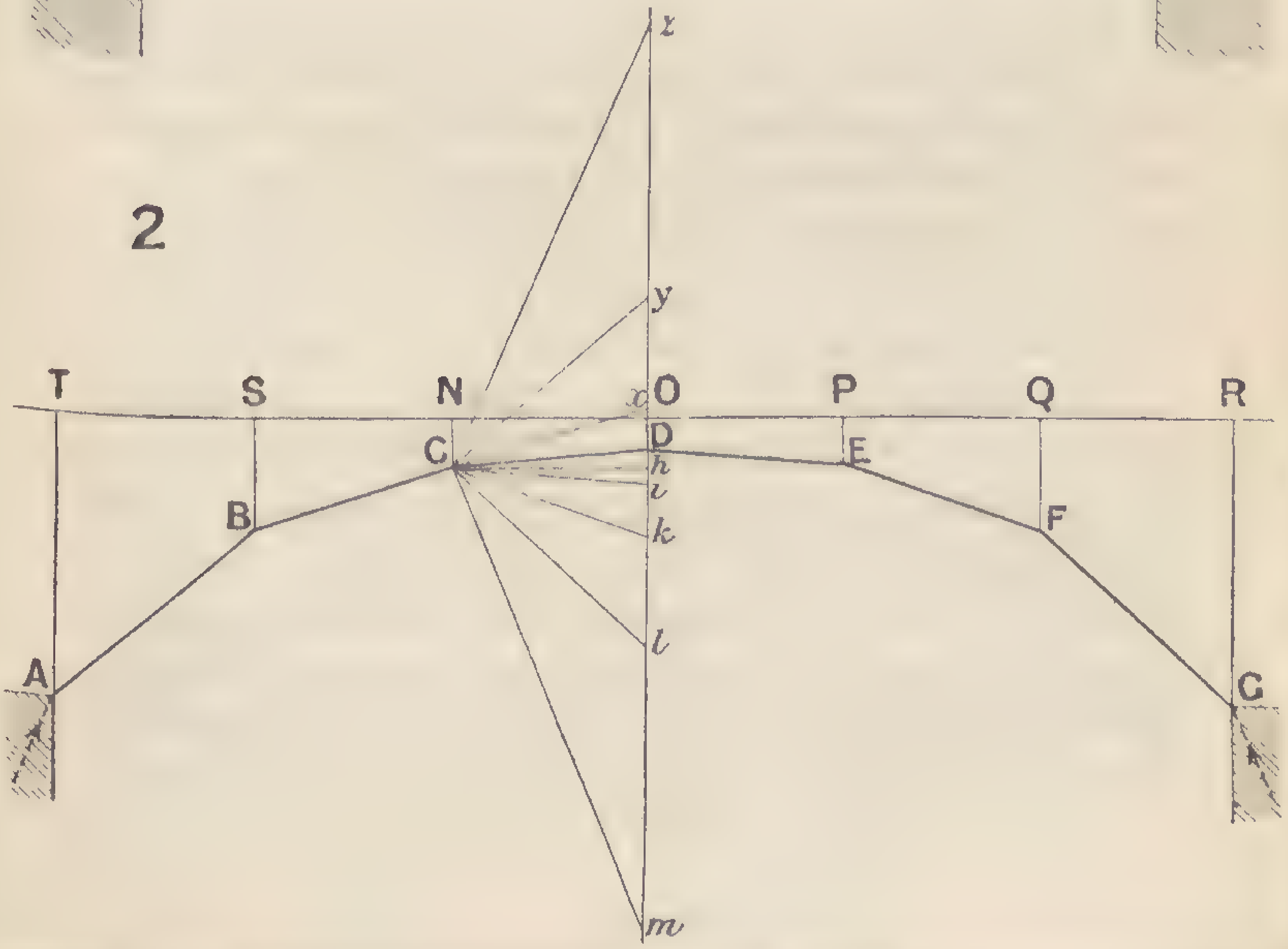

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ively to C B, B A and the dotted line through A; then C x, C y, and C z will represent the thrusts along C B, B A, and the dotted line through A.

Thus we have in this triangular frame z C m lines representing all the thrusts in the entire structure, those thrusts in the direction of the arch rods A B, B C, C D, D E, E F, and F G by C y, C x, C D, C i, C k, and C l, the horizontal thrust by C h and the vertical loads A T, B S, C N, O D, P E, Q F, and R G by $z\,y$, $y\,x$, x D, D i, $i\,k$, $k\,l$, and $l\,m$ respectively. The line $z\,m$ represents therefore the weight of the whole bridge.

From what has been proved by means of this diagram, it may also be shown that if we wish to sketch an arch of the correct form to suit a given span under a given road line, we can, by an inversion of the process just described, obtain the true form of the equilibrated curve, by construction on a sheet of paper, in the following manner:

For instance, let our object be to construct an equilibrated arch to carry a level roadway T R over an opening 30 feet wide between the abutments A and G in Diagram 2. Let us begin by dividing the roadway T R into six equal lengths of five feet each at T, S, N, O, P, Q, and R, and through all these points draw vertical lines. We next assume a crown thickness O D in the middle division of the roadway T R, let us say equal to two feet, being one foot for the thickness of the arch and another for the superstructure or metalling, etc. We then take D i equal to D O, and from h, the middle of D i, we draw the horizontal line h C, meeting N C in C, then the line joining C D will be the first rod, and D E, parallel to C i, will be the corresponding rod on the other side of the centre D, meeting P E in E. We then take $i\,k$ equal to N C or P E and join C k, and draw E F parallel to C k, then taking $k\,l$ equal to Q F we join C l

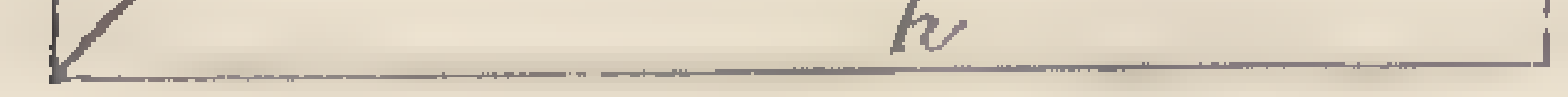
and draw F G parallel to it. Lastly, we take $l\,m$ equal to R G and draw the dotted line from G parallel to it. We have thus completed the arch on that side and may complete the other side in the same way, the two sides being naturally alike under a horizontal road line.

We have thus drawn a truly equilibrated arch between the abutments A and G, in which every line in the diagram shows its thrust in its own direction.

In the diagram we have only divided the road line into six equal parts for the sake of clearness, but in practice it may preferably be divided into a much greater number of equal parts, in order to show more exactly the true curvature of the arch.

This constructive method is perfectly true in principle; but in practice it requires so great accuracy that it can hardly be relied upon for arches of wide span, because the errors are accumulative. It is better, then, to make use of Dr. Hutton's formula in such cases, whereby we may obtain the true curve to any degree of accuracy.

In the next example the curve has been obtained by the use of this formula, a copy of which is here inserted:—



Crown Thickness $= a$
Half Span $= h$
Rise $+ a = c$

$$y = h \times \frac{\text{hyp.}\log \frac{x+\sqrt{x^2-a^2}}{a}}{\text{hyp.}\log \frac{c+\sqrt{c^2-a^2}}{a}}$$

The large drawing shows the elevation of a "Model

Arch,"—so called because the curvature of the arch is in the truly equilibrated form, and because it will serve to prove in a practical manner many points in the science of Arch-building.

The scale of this large drawing* is two-thirds of an inch to a foot (or an eighteenth of the natural size). The span of the arch is 85 feet, the rise 10½ feet, or an eighth of the span, and the crown thickness is 5½ feet up to the road line. The road line is horizontal. The thickness of the brick-work forming the arch itself is three bricks, or 27 inches; the crown thickness of 5 ft. 6 in. is therefore made up of 2 ft. 3 in. of brickwork, and 3 ft. 3 in. of superstructure or metalling, etc.

Being an "equilibrated" arch, the natural line of thrust passes exactly along the middle in thickness of the arch from one abutment to the other.

Now, taking the weight of the material, both of the brick arch and of the superstructure, to be 1¼ cwt., or 140 lbs., to the cubic foot, the load on the crown, which is 5 ft. 6 in. thick, makes the vertical load there 7 cwt. on the square foot. The thrust or bed pressure on the brickwork at the crown is 38½ tons on every foot in width of the arch, which is the constant horizontal thrust, throughout the whole arch; while at the springing, on account of the greater vertical load there, it will be 44·9 tons. The thrust at *any* point in an equilibrated arch is always the horizontal thrust multiplied by the secant of the angle of curvature at that point.

Again, taking the safe load upon the brickwork to be 5 cwt. on the square inch, or 36 tons on the square foot, the *necessary* thickness of the arch would be 12⅚ inches at the crown, and 15 inches at the springing; we may therefore take 15 inches as the necessary thickness of the arch

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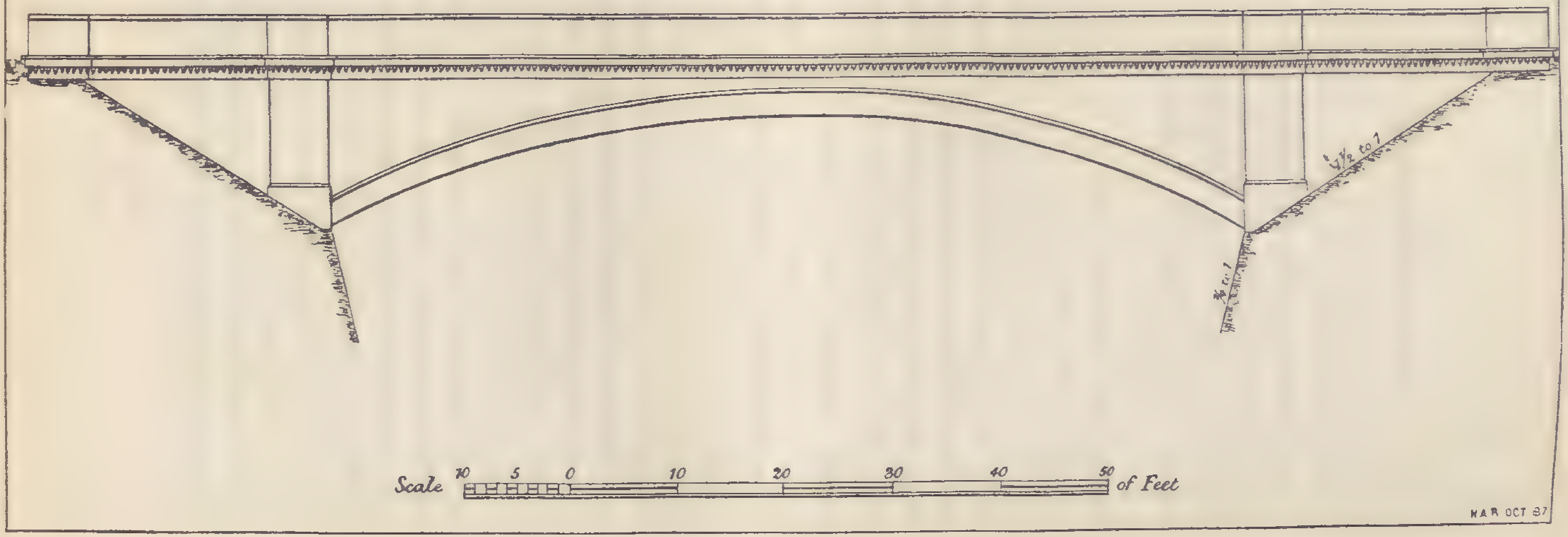
* See reduced diagram annexed.

—ROAD BRIDGE_OVER_WITH EQUILIBRATED ARCH—

—*Rise 1/8 of Span.*—



throughout, leaving a thickness of 12 inches as a margin of safety against any moving or additional load that may be placed upon it.

If a different form of curve in the arch had been adopted, that of a circular arc for example, the thickness of the arch brickwork must contain within it the needful 15 inches of thickness all round in the true line of equilibrated thrust; for Nature will follow no other, form the arch how you will. Now, the circular arc leaves the true line of thrust 6 inches at the haunches on each side; therefore, to make the circular arch equally strong, it must be made six inches *thicker* all round. This would, therefore, require nearly a quarter more brickwork, and then the arch would not be nearly as elegant as the natural curve.

That the curve of this arch is truly equilibrated may be shown in a simple and practical manner by *suspension*, as has been before described.

We have here a brass chain of the length of the curve as shown on the model drawing. Each link of the chain represents 9½ inches of the arch, and from each link is suspended a steel rod of a length representing exactly the calculated load upon that 9½ inches of the arch. The model being taken to represent one foot in *breadth* for the convenience of calculation.* The model is not *perfect*, but it is near enough to the truth to fairly represent the various points alluded to.

Let us now invert the drawing, and by means of these hooks hang on the chain, so as to bring the chain-ends to the points of springing. It will be at once perceived that the chain, as it adapts itself to the true theoretical line of equilibration, precisely represents also the curve of the arch as drawn, and the ends of the suspended rods show the horizontal road line.

---

* One ounce in the model represents 2½ tons in the bridge.

This is a practical proof of the accuracy of the curve calculated from the formula, and of its coincidence with the true *line of thrust.*

But this model will tell us more than this, and more than we can find out from our formula. For instance, though we may now be prepared to grant that the arch may at present be in true equilibrium as it stands, we may still wish to know how it would be affected by a very heavy load passing over the bridge from end to end. This the model will at once tell us in an equally simple and practical manner.

Let us take, for example, the heaviest locomotive engine, say of 50 tons weight, on a wheel base of 16 feet, passing over the bridge; what effect will such a load have in deflecting the curve of equilibration?

First, we must recollect that this wheel base of 16 × 5 feet on a permanent way would be spread, by the cohesion and friction of the structure, at an angle of at least 1 : 1, and that therefore the load would take a bearing on the arch below of 27 feet long by 16 feet wide; that is to say, that on 16 feet in breadth of the arch a new load of 50 tons would be placed, extending 27 feet in length. This would add 62½ cwt. to the load on each foot in breadth of the 16 feet. Now, as we suppose the chain model to represent one foot in breadth of the arch, then the imposition of this 50-ton engine will add a load of 62½ cwt. to 27 feet run of this model; and as the rods are 9½ inches apart, it will come on 34 rods with a load of 1·84 cwt. on each rod. We have here 34 small brass weights which represent 1·84 cwt. on the scale of the model, and if we stick these weights on 34 successive rods in any part of the arch, we shall see precisely the effect on the equilibrated curve, of the 50-ton engine standing on that part of the arch. Or, if we start from one end and stick the weights on the first 34 rods, and afterwards move them forward one by one, we shall see the effect of the 50-ton engine as it passes over the whole length of the bridge. We shall find that its greatest effect is when it gets near the centre, when it depresses the "line of thrust" $2\frac{2}{3}$ inches.

It may be well to pause a moment here in order to form a clear conception of what is meant by the "line of thrust" in an arch. For this purpose we may with advantage look first to the suspended arch; for its equilibration is self-formed and therefore both visible and measurable; we must also recollect, that every constructed arch has its suspended counterpart, in which the forces in every direction are the *same*, the nominal difference being, that what are *tensions* in the suspended, are *thrusts* in the "constructed" arch.

In the model bridge as drawn, the line of thrust may be supposed to pass through the centre, in thickness, of the arch; while in its suspended counterpart, the chain model, it passes through the centre of the chain; but this really makes no difference, for if the line of tension is supposed to pass through the rods, half the thickness of the arch below their tops, so that the tops of the rods should, when the model is turned up, represent the soffit of the constructed arch, and the line of tension be thus brought into the middle of what represents the arch thickness in the constructed arch, yet it would not have made the least difference to the curvature, or to the comparative thrusts and tensions anywhere, but it would have added a great difficulty to the manufacture of the chain model. The model, as it stands, is not really perfect; for example, in our calculations from the formula, we assume the distances apart of the rods to be the same *horizontally*, but they are not so, for the links of the chain are of the same length all through, and this brings the rods nearer together towards the springing, where the chain becomes gradually more inclined. This, of course, makes the vertical load a little too heavy as it approaches the springing; but in an arch like this, of wide span and small rise, it makes so little difference that it is not worth notice.

Returning again to the "line of thrust." If an additional load be placed upon any part of the arch, the alteration in the line of thrust is at once apparent in the altered *form* and *tension* of the chain of the suspended arch, which adjusts *itself* to the new equilibrated curvature; but it is not apparent in the constructed arch, notwithstanding that the line of thrust *is* similarly and equally altered in both. The chain, in fact, gives us the means of knowing, mechanically and precisely, what this alteration amounts to in the forces affecting the solid arch.

The alteration in the "line of thrust," as passing through a constructed arch without outwardly showing any visible change, may be illustrated by a familiar example. Let us suppose that a stone pillar has been standing for some years in a builder's yard. As it stands there, the "line of thrust" is vertical from the centre of gravity on to the slab on which it stands, the weight of the pillar giving the amount of the thrust and the size of its base the margin of stability. Being out of the way, and handy for his purpose, let us suppose a workman tilts a couple of planks against it; the pillar makes no sign, but its "line of thrust" is altered by the push of these planks, and brought a certain amount nearer to the opposite angle of the base. By-and-by the workman tilts against it two more planks, and the thrust line is brought another half-inch nearer to the angle of the base; but yet there is no movement, for the point of thrust is still within the base. Seeing no movement in the pillar, the workman occasionally adds another plank or two, until, in the end, the last plank he adds sends the line of thrust *outside* the base, and then the whole falls to the ground.

This exactly represents the relation of the "line of thrust" to the constructed arch. The arch, once built, cannot move; it can only tumble down whenever the "line of thrust" is driven *outside* of the arch in any part.

In the chain model, therefore, we see that the chain (every link of which is freely movable in any direction in answer to the forces applied to it) must accurately assume the truly equilibrated curve-line of tension, and *must* also thus show us the corresponding "line of thrust" in the constructed arch, both as it stands under the dead load only, or under any added load. In the same way we have seen that the weight of *each* rod and its link tells us the precise *vertical* load at that point. Thus we get the load of 7 cwt. on the square foot at the crown of the arch represented by the weight of the central rod and its link. We get the thrust against the abutments, 44·9 tons on the foot in width of the arch, by the pull of the chain at its extremities, as measured by a delicate and well-constructed spring balance; and the horizontal thrust throughout the arch, 38·5 tons on the foot in width of the arch, by the pull of the chain in its centre, where the chain is horizontal.

We may also get, in like manner, exact measurements of all the increased strains or thrusts caused by any *added* load on any part of the arch; such as those that would be caused, for example, by the passage of the 50-ton engine over the whole length of the bridge.

Now, as in the suspended arch, the chain must be made strong enough to bear the dead load of the whole structure, together with the greatest live load that can ever be placed upon it; so must the thickness of the constructed arch be sufficient to bear the constant dead load of the whole structure, together with the greatest live load that can ever be taken over it.

Taking first the case of the *dead* load. In the model arch it has been shown that the thickness required for this purpose must be $12\frac{5}{8}$ inches at the crown, and 15 at the springing, tapering proportionally from the one to the other.

This I have termed the *necessary* thickness, for it forms the basis of our reckoning, as well as the main consideration, particularly in arches of wide span. If, therefore, on the drawing of the arch (Plate XVII.) we mark off a *middle* thickness of $12\frac{5}{8}$ inches at the crown, and of 15 at the springing, and draw lines in the proper curves,—A the upper and B the lower,—joining these marks (the thickness is at every point directly proportional to the secant of the angle which the equilibrated curve makes with the horizon at that point). Then, this middle space will show the *necessary* thickness throughout the length of the arch. This is on the assumption that the safe load on the materials should be one-eighth of the crushing load of cement, as will be alluded to presently.

Having now provided for the *dead* load by means of that middle portion of the arch between the lines A and B, let us look more particularly into the effect of the *live* load. This has been represented by the supposed passage of the very extreme load of a 50-ton engine over the bridge, and the results have been exemplified practically by its effect upon the suspended chain model. We have seen that the chain (which constantly represents the equilibrated curve of tension) was always deflected under the engine as it passed along, until it attained an extreme deflection, in mid span, of $2\frac{2}{3}$ inches. At the same time that the chain was deflected under the load at any point, it was seen that the radius of curvature of the chain was *sharpened* there, under the load, but flattened along the other parts. This latter effect we at once know *must* be the case, for the chain would otherwise have been lengthened.

If now, while the engine is in mid span, we prick through every link of the chain into the paper behind, we shall obtain the new line of equilibrated curvature, and shall see exactly how the equilibrated curve of the line A has been modified by the added load of the 50-ton engine on the middle of the arch. Let us draw a dotted line through these points, and then turn the drawing the other way up. Now consider what we get then. We get the true form of the constructed arch under the *new* load, as pointed out by the hand of nature, and we know that the line of thrust must assume that new form in order to support the combined loads *now* placed upon it. We therefore see that, in order to carry in truly balanced form this 50-ton load on the middle of the constructed arch, the equilibrated curve of thrust is there *raised* $2\frac{2}{3}$ inches, though the curvature is *depressed* somewhat along the other parts, which are not similarly loaded; the dotted line will accordingly be $2\frac{2}{3}$ inches *above* the line A at the crown and gradually run into it lower down. We know that the line of thrust, under the load, *must* and *does* pass along a line represented by that dotted curve through the solid arch, though nothing of this is visible in the constructed arch.

Again, we know that under the 50 tons added load there would also be an increased *amount* of thrust, which would render necessary an increased thickness of arch. Let us reckon what this increase amounts to.

As the engine stands upon the crown of the arch, it would of itself, as has been already explained, add 2·32 cwt. to the 7 cwt. load on the square foot at the crown. This, multiplied by the radius of curvature, which is now 104 feet, as shown by the dotted line, instead of 110 feet as formerly, will give us an additional horizontal thrust of 10 tons on the foot in breadth of the arch. This additional thrust will add $3\frac{1}{3}$ inches to the necessary thickness of the arch at the crown, and 4·4 inches at the springing. If these additional thicknesses are added on to the original necessary thickness, they will make $12\frac{5}{6}+3\frac{1}{3}=16\frac{1}{6}$ inches at the crown and $15+4{\cdot}4=19{\cdot}4$ at the springing, as shown by the lower dotted line under the line B, they will give us the necessary thickness under the new load all round, and will still leave an ample safety margin of about 8 inches everywhere.

It has been asked, What would be the effect of a *train* of such engines standing upon the bridge?

To show the effect of this by means of the chain model would be simply to stick upon *every* rod throughout the model one of the small brass weights used to represent the load of the 50-ton engine. The effect of this is to practically restore the original *curvature* of the "line of thrust," and to add a *little* to the horizontal thrust. Thus, putting it into figures: The original radius of curvature of the "line of thrust" at the crown was 110 feet, and this was *sharpened* to 104 feet by the 50-ton engine, while the rest of the chain was flattened. This fact proved simply that, as compared with the original line of thrust, the *proportion* of the vertical load to the horizontal thrust had been increased at the crown and diminished elsewhere; but now, by the addition of the increased weight to every rod *all along* the model, we have practically restored that proportion, and there is now left only the increased horizontal thrust to be taken into account. This would therefore make the total load at the crown 9·32 cwt. on the square foot; and this multiplied by the radius, 110 feet, would give 51·26 tons on the foot in width of the arch, rendering necessary a thickness of 17·1 inches—that is, an additional thickness of 4·3 inches; but, as the original curvature would have been restored, this takes off the *deflection* of the "line of thrust" before alluded to,—a less trying load than that of the single engine,—*this* tries the *equilibration* of the arch, the other only the *crushing strength* of its materials.

Before we leave this question of general remarks on the "line of thrust" it may be well to consider the general stability of arches, whether built of the true curvature or not, from another point of view; namely, that of what may be called the destructive load.

Let us suppose that, for experimental purposes, we are going to load a well-built arch until we break it down; and let us further suppose that, with this object in view, we pile up the load continually upon the crown of the arch until we attain that result; the arch having been built, we suppose, to carry safely a given load, and loaded up to that extent before we begin to add gradually the destructive load. We may have drawn beforehand the lines which we have called the necessary thickness; and as each increment to the load is added, we may add also to the curvature and thickness of this necessary arch the theoretical amounts due to the loads just imposed, which, being on the crown, will keep on raising and sharpening the curvature of the line of thrust, as has been described in the former case, until at last the curvature gets outside the lines of the arch. We then, in accordance with our theory, expect that the arch will fall from dislocation or want of balance, and not from the crushing of the materials, because the additional loads have all been added on the crown. But it must be recollected, that although our equilibrated line of thrust may have been correctly drawn, the arch will not yet give way unless we have at the same time drawn it in the *strongest* equilibrated form that the arch as constructed will admit of.

Let us now take a glance at the question of *fancy* arches; that is, of arches built in some fanciful form in order to please the eye, but not in the truly equilibrated form.

Take first the flat Gothic (see plate XVII.). If an arch of this sort is to stand, we must first draw the strongest equilibrated arch we can get into the pattern, and having made it of the necessary thickness to carry the superimposed load, with a fair margin of safety, we can add both above and below this such additional thicknesses as we wish in order to give the Gothic form to the structure. In the sketch the dotted lines show the real arch that bears the load, and the full lines the pieces added to give the required form. These last merely signify so much more load when added underneath, but the "line of thrust" still passes along the dotted arch.

In the semicircle the equilibrated arch under a horizontal load-line would pass into the abutments much above the nominal springing, and the "line of thrust" would then leave the apparent arch and pass into the backing. The angular pieces put in to complete the circular form would be merely suspended from the *real* arch above; but the bedding of the arch stones should be square to the "line of thrust" and not to the circle.

The platband, properly constructed, contains the true arch principle. Let us suppose we wish to make a drawing for the best form of platband, having a span of 6 feet and a height of 2 feet (see plate XVII.). AB is 2 feet and DE

6 feet. Divide AB into eleven equal parts, and call the sixth from A, P; thus AP $=\frac{6}{11}$ and PB $=\frac{5}{11}$ of AB. Then describe the arc of a circle passing through the points D, P, E, having its centre in C. Also from the same centre describe a parallel arc passing through A. Then the space inclosed between the two arcs will be the strongest arch that can be made within the limits of the platband, and all the joints of the masonry or brickwork should be made to radiate from the centre C.* The true arch lies between the circular arcs; the prolongation of the arch-stones below that line are merely suspensions from the real arch above, and do not add in any way to its strength.

Let us now proceed to the consideration of the strength of the materials of which the arch has to be built; namely, stone or brick and cement.

On first thought, it would be supposed that a good building stone would be the strongest and best material for arch building; but the determination of this question is usually very much influenced by that of *cost*. If in building an arch all the stones were dressed to the true wedge form from front to back, and then fully bedded *throughout* in cement, a good stone would undoubtedly form the best arch; but, practically, this cannot be done, for two reasons: Firstly, no one will go to the expense of using fully-dressed stones all through the arch; they will dress the stones on the *face* of the arch from front to back for the sake of appearances, but the middle parts of the arch, which really have to bear the load, are usually filled with rough *backing*, and are only *faced* with dressed stone. The main body of the stone arch is therefore a mere face of dressed stones backed up with rough stonework and a much larger proportion of mortar; but the mortar is much the weaker material of the two. Perronet, indeed, who built in Paris, about the end of the last century, some of the most bold and beautiful bridges yet constructed, did dress his stones for the full thickness of the arch from front to back all through; for he had access to splendid stone quarries, and had the State for his paymaster. From his drawings it can be seen that he commonly used stones 5 feet long, 4 feet deep (the thickness of his arch), and over a foot in thickness, in the middle parts of his arch. But now comes the second reason, you cannot *bed* these large stones *throughout* in mortar or cement. The usual way of setting these large blocks of stone is by slinging them over the spot from a crane, spreading the mortar with a trowel as evenly as you can on the proposed bed, and then letting the stone down into its place; but the stone cannot be thoroughly bedded in this way—it bears hard in places, but does not touch in others, as may be proved experimentally if the stone be raised again a day or two after, when the places where it did not touch will become apparent. It is in fact a most difficult operation, to thoroughly and completely bed a large block of stone, and in such a case as this, with highly inclined beds, practically impossible. Engineers of the greatest experience say that it is impossible to bed a large block of stone with a trowel.

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* The circular arc, in so small a segment, represents the equilibrated curve as nearly as is practicable.

Now, considering that the best cement will not bear a crushing weight of more than two tons on the square inch, while the best class of stones will bear double that weight, it is clear that the *cement* is the weakest part of the arch, and that consequently it is of the greatest importance that it should have a complete and thorough bearing.

On the other hand, let us see how it is with brickwork.

In discussing the comparative strength of a brick arch, it must be premised that the arch is supposed to be built with a perfect *vertical bond*, and not in rings, as has been too commonly and unscientifically practised.

Now, a good and thoroughly well-burnt brindle or vitrified brick is as strong as any stone, and will bear a crushing weight of four or five tons on the square inch; therefore, with this material also, the cement will be the weakest part of the arch, and therefore the limit of strength. But the advantage of the brick is, that it is handy, and *can* be thoroughly bedded, quickly and without difficulty, in the cement. This gives the weaker material, the cement, a *full-bedded* bearing, and for that reason makes brickwork superior to stonework in building an arch which has to carry great pressure, even if the stones are dressed to the full thickness of the arch as done by Perronet.

Thus good brickwork forms the strongest arch, as it is also by very far the cheapest.

In a properly constructed arch such as has been described, let us consider for a moment the limit of span.

I have said, on a former occasion, "I think that engineers have been very bold in ironwork and very timid in brickwork."

The ironwork is considered safe under a load equal to a *quarter* of its breaking strain; but brickwork is supposed to be safe only under an *eighth* of its crushing weight. This small limit to the safe strength of brickwork has probably arisen from the false construction of the brickwork. I have known several bridges to fall down from the *dislocation* arising from false construction, but never in any case from the crushing of the materials; for when the arches alluded to fell, the bricks were as sound as on the day when they were put in.

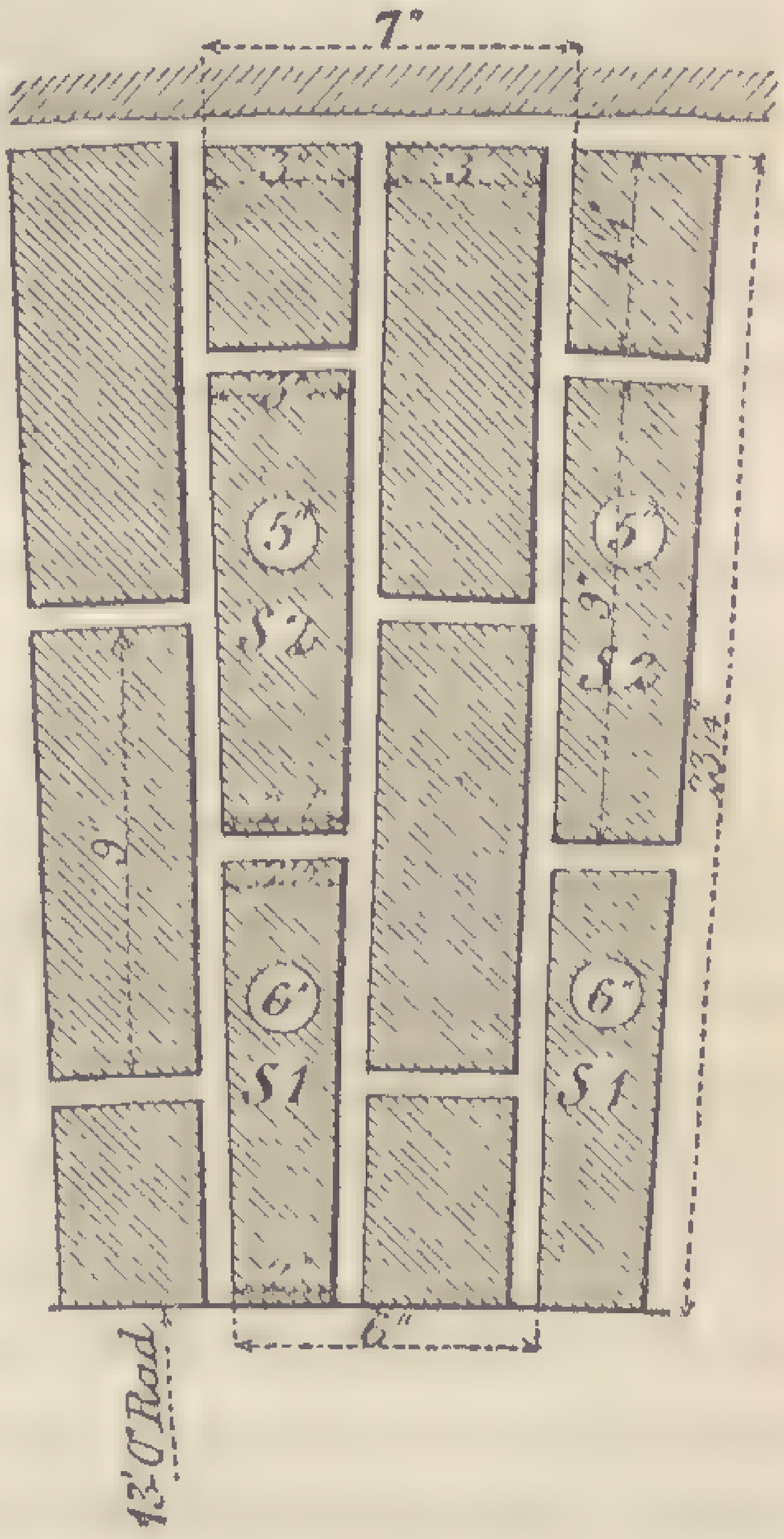
Comparing the two rules of safe load just mentioned, it is known that with an iron beam, if a heavy load, though well within its strength, is continually put upon it and taken off again time after time, the breaking strength of that beam becomes less and less; but in the case of a mass of brickwork, if a similar load is put on and taken off ever so many times, the brickwork will become no weaker, for there can have been no disarrangement of its particles either by deflection or from any other cause. It would therefore appear that the *safe* load might be taken *nearer* to the destructive limit in materials that have to bear a simple crushing weight than in an iron beam which has to sustain a movable deflecting load, which causes the iron to bend, and which must, therefore, disarrange the particles every time. But taking the safe load as only an eighth of the crushing load on cement, or 5 cwt. on the square inch, and taking the model arch before mentioned as our example, it may be remembered that we found that an arch 15 inches thick at the springing and $12\frac{5}{6}$ at the crown, was sufficient for a span of 85 feet with a rise of one-eighth of the span. As all the loads and thrusts in such an arch are in direct proportion, if each dimension in the model arch were multiplied by four, we should have a span of 340 feet, with a rise of 42 feet, and a necessary arch thickness of 5 feet. This 5 feet thickness of arch would also of itself leave a sufficient margin of safety for the moving load, because 5 feet is only the necessary thickness at the *springing*, while that at the crown would be nine inches less. This at once forms a 9-inch margin of safety at the crown, where the moving load, it will be remembered, had the greatest effect, causing in that case a deflection there of $2\frac{2}{3}$ inches, the deflection being 0 at the springing, and gradually increasing to 2·67 at the crown. Now the total weight of that bridge was 8,350 tons; but one of 340 feet span would have a weight of

100,000 tons, and the 50-ton engine passing over it would have a very slight effect on so heavy a structure. The deflection at the centre would be less than half an inch, therefore the nine inches of margin would be ample.

With an arch 6 feet in thickness, the span might be 400 feet. These figures are calculated on the assumption that the safe load on brickwork is limited to *one-eighth* of the crushing weight on cement. If what is considered the safe load should be extended to *one-quarter* of the crushing weight, as is done in the case of iron, then the same thicknesses of arch given above would be sufficient for *twice* the span mentioned.

The chief argument against great spans lies in the cost of the centering. But a complete centering from springing to springing would be very seldom wanted; it is generally quite easy to divide the support of the centering into a number of sections—the more the better within certain limits. Not only for saving the great cost, but also because if the span is a wide one the centering must be *framed* together with timbers of moderate length.—Now, when it is recollected that this centering has to bear the weight of the whole arch without shrinking (for if *any* shrinking occurs the curvature of the arch must be immediately impaired), how can we expect that the struts in a self-supported, framed centering will not be driven further into their housings, and all the joints throughout the centering closer together by the imposition of this enormous load upon centres that have not been loaded before? It would therefore be better to have a line of props at every 20 feet if a good base can be got for them. It would save the large cost of a framed centering, and we should have one that could not sink under the load. Centering with a number of intermediate supports would therefore be far better, as well as much less costly, for arches of wide span.

The vertical bond in a brick arch I have usually formed in the following manner: Referring, for example, to a tunnel arch of 13 feet radius and 2½ bricks in thickness, as shown in the sketch, two special or radial bricks are required, marked S 1 and S 2 on the sketch, in combination with ordinary bricks, which we will take to be 9 inches long, 4¼ inches broad, and 3 inches thick, with half-inch joints of mortar. The size of S 1 would be 9 inches long, 6 inches broad, and tapering in thickness from 2 inches at the lower end to 2½ at the top; that of S 2 being 9 inches long, 5 inches broad, and tapering in thickness from 2½ to 3 inches at top. These special bricks would be of the same cubical contents as the ordinary bricks, and could be made in large quantities for five shillings a thousand extra.



Referring once more to the sketch, it may be observed how the bricks are laid forming a full half-brick vertical bond, the mixture of common and special bricks shown in two courses of the sketch making together one block of the brickwork (two such blocks are shown in the sketch) exactly fitting the curvature of the arch, six common bricks being used to every four specials; and as a thousand bricks will make three yards of brickwork, the additional cost would only be eightpence a yard over the whole arch. The fact of the special bricks being made of different *breadths* from the ordinary bricks is useful in order to break the horizontal joints, and it besides enables the men readily to distinguish them; they call them the 5-inch and 6-inch bricks, and cannot make any mistake in using them.

But in flat arches, like that shown in the model, special bricks are not wanted in order to form the vertical bond, for the tapering of each course can then be formed in the cement. In the model bridge, for example, the tapering on the whole thickness of the arch, 27 inches, amounts to no more than an eighth of an inch, which may be managed by making the joint a sixteenth of an inch closer, on the centering, and the same amount wider at the back.

In actual practice I have never found that special bricks are needed in an arch which has a curvature of 30 feet or more in radius.

Before leaving the subject of brick arches, it will be as well to say a few words as to arches built in *rings* of brickwork. This system, which has been so common in this country (though not now used abroad), has been adopted no doubt in order to make use of nothing but *common* bricks. The origin of this system may have been, that before the Excise duty on bricks was abolished there was great difficulty put in the way of making any other than *common* bricks; for large or special bricks were forbidden by the Excise; but now there is no such difficulty, and there is consequently no excuse for using them where taper bricks would do better.

In a ring-built arch each ring is a separate 4½ inch arch, and the entire arch is made up of so many distinct 4½ inch arches built one over the other. This is practically proved the moment there is any settlement in the arch or its abutments; the rings part company immediately, and nothing but friction keeps the arch up—the real arch principle is gone. Then, again, when the curve of the arch varies at all from the equilibrated curve explained above, if the natural line of thrust passes from one ring into the next, the arch would fall at once if friction did not prevent it. In arches of small span it does not so much matter, but in arches of large span it is fatal. Arches of wide span cannot be built with safety in *rings:* for example, referring to the model, the natural line of thrust must then pass round through each single 4½ inch arch; but if that arch were built in the form of a circular arc, the line of thrust would, as has been stated, leave the ring courses for a distance of six inches, and the line of thrust would pass from one 4½ inch arch into its neighbour; and under the great thrust due to a wide span the rings would slide upon one another, the haunches would rise, and the crown would droop until the arch fell. Even if built upon the true equilibrated curve, its stability would be very precarious, so slight a margin being left for any little settlement or other accidental defect.

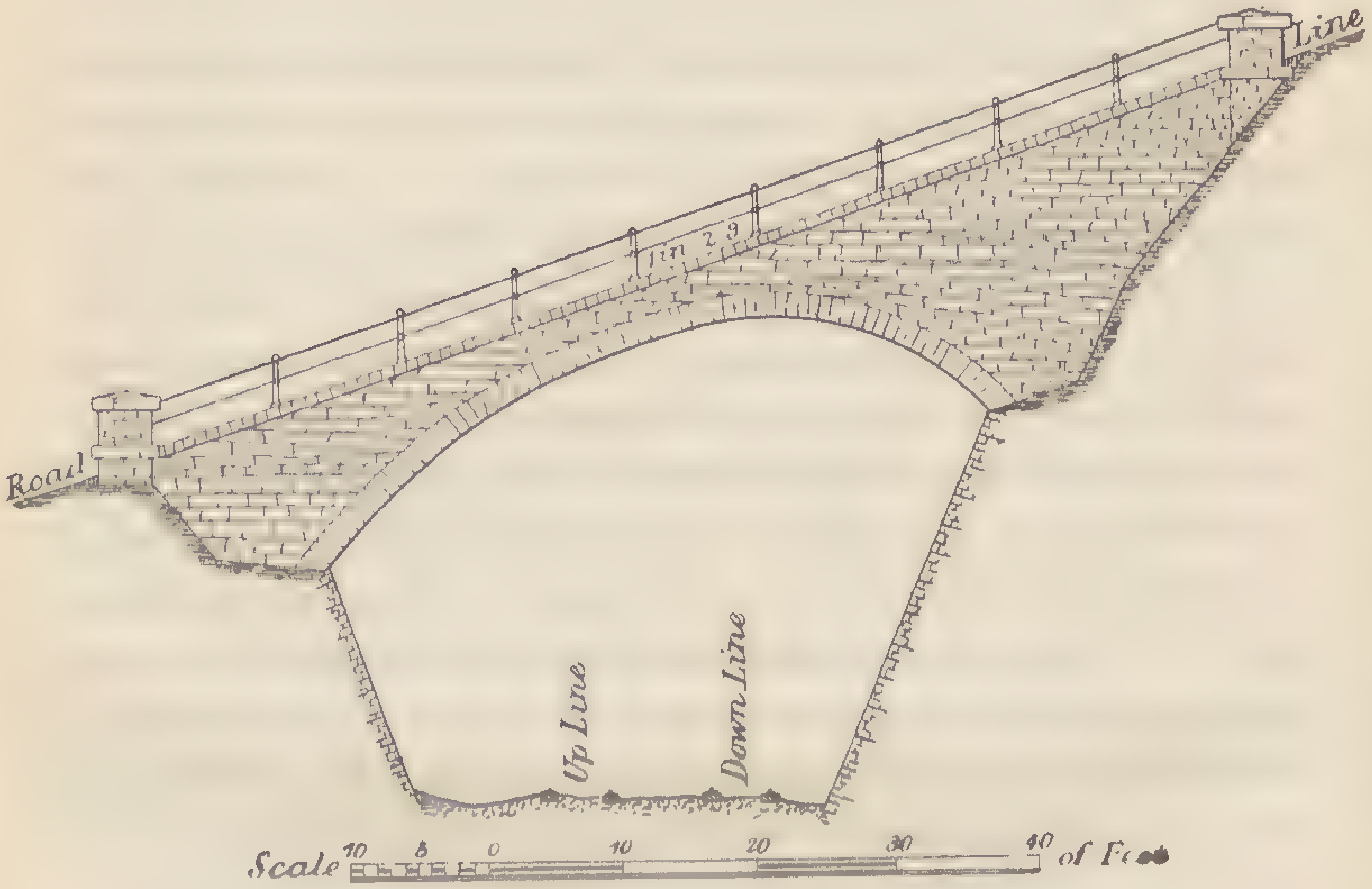
The almost universal adoption of the ring system of building arches, together with the use of unscientific curvature in the form of the arch, with the consequent settlement or failure of many of them, may probably account for the timidity of engineers in adopting brick arches of wide span.

A semi-circular arch under a horizontal roadway is always wrong; the natural line of thrust must always pass out of the arch into the backing, and if the arch stands, it is by friction only and by the good quality of the backing which has to sustain the heaviest part of the thrust.

That friction is a great power there is no doubt, as is proved by many instances, a few of which will be mentioned

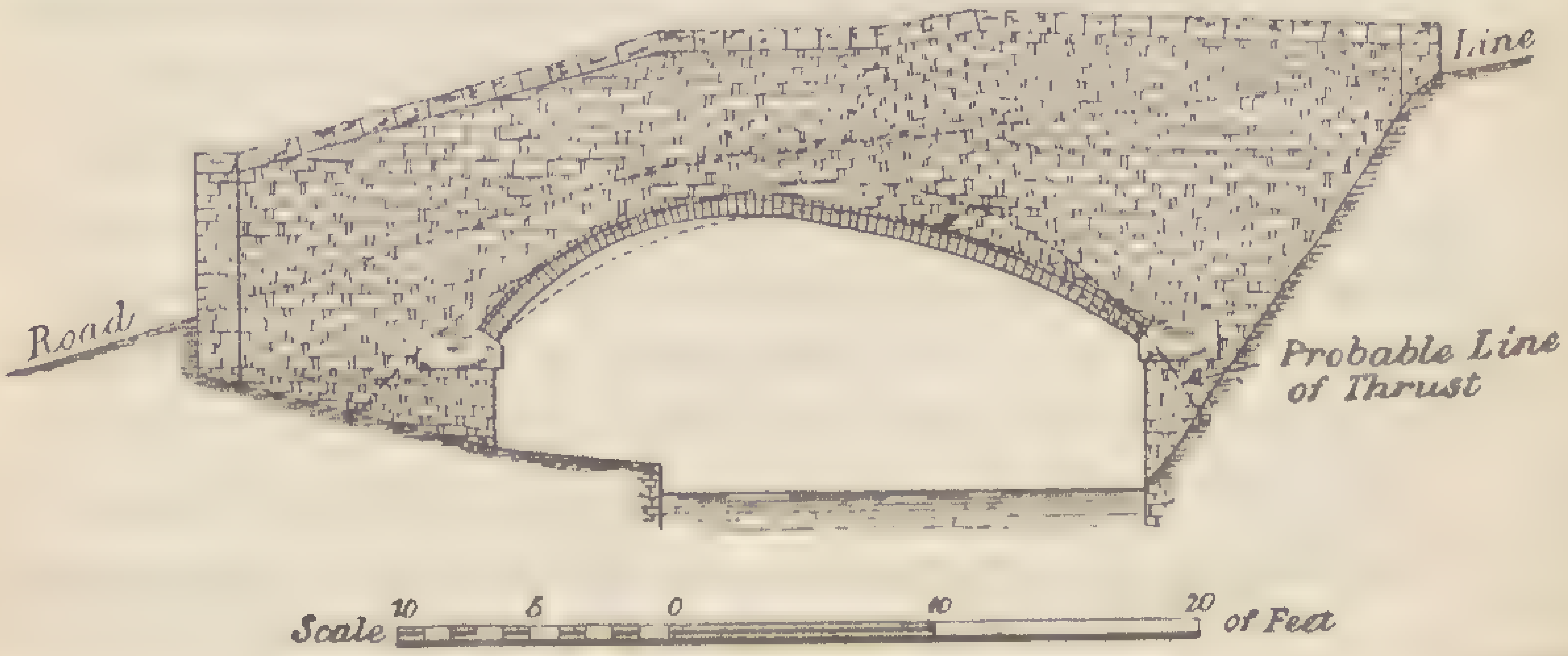
BRIDGE AT WESTLEY WOOD

CHALFORD VALLEY BUILT IN 1844.



BRIDGE OVER CANAL AT CAPEL'S MILL STROUD

BUILT ABOUT 1800 _ ARCH IN VERTICAL BOND



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presently. But it is neither scientific nor safe to trust to it, particularly in arches of wide span, in which the thrust is great and in direct proportion to the radius of curvature. At the same time, when an arch is *truly* built and with sufficient abutments, the flatter the arch is, the stronger it will be and the less likely to be disturbed, so long as the thrust is well within the strength of the materials.

A "skew" arch has always to depend largely upon friction; and it is doubtful whether there is any advantage obtained in building them in what are called "spiral" courses. Nature does not altogether accept the "skew," but the thrust always comes heaviest upon the obtuse corners, as is proved by the constant tendency of the acute, long corners, to droop; and if two skew arches spring from a centre pier, they will have a tendency to twist the pier round nearer to the square.

The finest proof of the power of *friction* that I have met with, is shown by a small drawing recently made of a small bridge over the Stroudwater Canal, built about the beginning of this century. At the time, in 1844, when I was looking after the construction of the Stroud Valley Railway, which passes near it, I used often to stop and gaze with wonder upon this arch—wonder that it should still be standing. The arch was, when built, of 24 feet span and $4\frac{1}{2}$ feet rise (the dotted black line shows the original form) with an arch-thickness of 9 inches; but, note here, it was built with *headers*, forming a vertical bond, and *that* chiefly gave it the strength to be still standing. It was built under a steeply-sloping roadway, and under this load the arch was very far from being in equilibration; the heavier haunch shoved up the lighter, and the arch must, by degrees, have assumed its present form, for it has not altered during the last fifty years. On the drawing (page 126) may be observed a dotted line showing the natural line of thrust, under the inclined roadway, which passes most nearly to the present arch—it will be seen that this line of thrust passes completely and largely *outside* the arch in two places; it is certain that the line of thrust must follow some such curve, and therefore that it goes for a long distance through the earth filling on the top of the brick arch, and that this earth has been jammed together so tightly that it forms that portion of the arch of itself; that it then passes into the brickwork of the arch again, and then again that it passes out of the brickwork once more into the earth backing at the point of greatest thrust—namely, the lower springing, the earth backing forming that abutment of the bridge. It may be added, that where the old arch at one part had become nearly a straight line, an iron plate has been added underneath, to keep the bricks from tumbling out, proving that there was no *thrust* in that part of the arch itself. The Canal Company is *very* poor, or they would probably have rebuilt the arch; but it is worth preserving as a great curiosity.

Under a *sloping* roadway an arch ought to be built with a corresponding variation of the curvature, as shown in the drawing of a bridge (page 126) which I designed and built at that time over the railway a few miles higher up the valley. The roadline there is steeper than 1 in 3, and a square arch would certainly have fallen. The span is 50 feet, and the crown thickness 4 feet; but one abutment is 12 feet higher than the other. The bridge is built of Bisley common stone, and is in the truly equilibrated form.

It may have been observed how very unsightly a *square* arch always looks under a *sloping* roadway.

A word more about friction. In Molesworth's well-known *Engineers' Pocket-book*, rules are given for determining the necessary thickness of an abutment to carry the thrust of any given arch. There, abutments are supposed to be built of the required thickness according to the height, in ordinary brickwork or masonry; the mass of the abutment being sufficient to bear the thrust of the arch. The consequence is, that the arch is always shoving at the abutments, and there is nothing but the friction of the courses to resist this action, with the result that where there is much jar from heavy waggons passing along a street alongside, such arches frequently settle and open at the joints more or less in the course of time. An instance may be observed in the façade of the old station front built at Temple Meads by Brunel years ago. It may be recollected that an entrance and an exit archway were built at either end of the façade—one under the clock which used to be there, and the other, which has recently been pulled down by the Tramway Company. Now these arches were only of thirteen feet span and all the masonry was splendidly built —a full thickness of abutment, according to rule, being allowed to both arches; but I had noticed for years that openings and settlements in the joints of both of these arches had taken place (the worst being in that on the tramway side). These settlements were caused gradually by the jar of the passing wagons on the street pitching. Those on the clock side have been pointed over on the front façade, and are somewhat hidden, but at the back they are more visible. Now we can tell precisely the amount of horizontal thrust of these arches against the abutments; and if the abutments had been built with a certain amount of countervailing *batter* which was also loaded on the top, then one thrust might have been made to balance the other, and there could have been no settlement of that sort. This battering wall need not appear on the outside, for it may there have been *faced* with horizontal courses of masonry. This is not a glaring instance, but I have selected it as being near home.

Maidenhead Bridge.—The last of the drawings (not reproduced here) shows an elevation of this bridge, which was built over the Thames, for the Great Western Railway Company, by Brunel. The two river arches have each a span of 128 feet, with a rise of about one-eighth of the span and a crown thickness of 6 feet. The bridge is built of brickwork, and the arches in *rings* of brickwork.

It is a beautiful bridge, and is chiefly remarkable for the last-mentioned fact, which makes it of interest in this paper.

The drawing underneath is that of a bridge as nearly similar as possible, but built with equilibrated arches. It is remarkable that the curvature of the arches is the same in both bridges along *two-thirds* of the span; the old arches only depart from the true curve when within 22 feet of the springing, from which point they are turned downwards so as to give them an elliptical appearance. The actual line of thrust, however, always follows the direction of the truly equilibrated curve, shown in the drawing below.

This is probably the boldest bridge ever built in *rings* of brickwork. Had not all the central portion of the arches been in the true form of curvature, and had not the parts near the springing, where the true curvature was not followed, been made immensely thick and solid, so that the natural line of thrust could pass out of the arch into the solid mass of brickwork at the back, it could not have stood. This, however, involved the employment of a large quantity of additional brickwork, not otherwise needed, and the work was, after all, not so strong as the truly-formed arch would have been *without* this additional brickwork.

The bridge had, in fact, to trust largely to *friction* at the haunches, where the "line of thrust" passed from ring to ring and, eventually, out of the arch into the backing.

If this arch had been built in the true form, the *necessary* thickness (as before described) would have been three bricks, and if a fourth were added as a margin of safety, the arch would have been four bricks, or three feet thick all round; but, as built, the arch is 5 feet 3 inches at the crown, and from that point getting gradually thicker till it is 7 feet 9 inches at the two-thirds point before alluded to, and it then springs from a block of brickwork 20 feet thick, with an immense basis carried down to the chalk 16 feet below the surface of the ground.

Notwithstanding this strength of work, the arch followed the centres the first time they were eased, and a good portion had to be rebuilt, so that there was a great doubt in the public mind if the bridge *would* stand, until the question was set at rest by the centres (which had been slacked for some time) being all blown into the Thames by a heavy gale of wind.

The great fault in the construction of this bridge was, without doubt, the building of the arch in *rings* of brickwork—it would otherwise have been more than amply strong.

Alluding once more to economy and strength in centering, this bridge forms a good example of what has been said before on this subject. It would be absurd to erect entire self-supporting centres for these arches. The necessary opening for the passage of boats is very small, as shown by the small arches in the road bridge just above; and it would make better, as well as cheaper, centering to have it supported from several rows of piles in each case, for the reasons explained above.

In conclusion, I may say that I think Brickwork has not had its fair chance in comparison with Ironwork, particularly of late, as a material for bridge-building in general —from ordinary road bridges up to arches of a large span in suitable situations. Brickwork, if used with taste and correctly applied, will be found to excel Ironwork largely in strength, durability, and economy, as well as in outward appearance.

On the Setting of Steam Boilers.

BY CHARLES J. SPENCER.

Read November 15th, 1887.

IN choosing for my subject "The Setting of Steam Boilers," I am not laying claim to any special knowledge of the subject, more than is possessed by many others here, who, doubtless, have had more experience in and studied the subject more than I have done.

At the same time, it is a subject which Engineers have not paid so much attention to as they might have done, perhaps through no fault of their own, but partly because it is thought to belong more to the province of the bricklayer and mason, and partly because the boiler user may have ideas of his own on the matter, and is afraid the Engineer will reap a small profit for himself, should it pass through his hands.

In my opinion, it is almost as important to a steam user that his boiler should be well *set*, as that the boiler itself should be a good one; and I am sure it would be to his advantage to employ a competent Engineer, not only to inspect his boiler, but to design the setting and see to its being carried out in a proper manner.

Now, the most natural inquiry at the outset is, What is the object of the setting of a boiler, and what are the conditions under which that object is best attained? The object is, to bring the flame and heated products, generated in the furnace, in contact with the surface of the boiler; and it is best attained—(1) when the greatest amount of surface is exposed to the flame; (2) when the greatest amount of exposed surface is horizontal, or in the most favourable direction for the flame to impinge upon it; (3) when the passage of the gases through the flues is least impeded by contracted area or by obstructions in them; and (4) when the gases can be kept in contact with the surface long enough to part with all or practically all their heat without checking the draught into the furnace.

Keeping these points in mind, I will now notice the most usual forms of setting in vogue, as applied to the two types of land boilers almost universally used—the Lancashire and the Cornish types. I may dismiss the Egg-end boiler as almost obsolete, except in the case of rolling mills, where it is heated by the waste heat from the puddling furnaces, and in the case of collieries, where coal is so plentiful that it matters little what kind of setting is adopted.

The most usual forms of setting are but two—the *split* draught and the *wheel* draught.

Regarding these critically, to find out their weak points, as it is the Engineer's business to do, you will notice the draught is split either at the back to traverse the side flues, or, as is most usual, it traverses the bottom first, is split at the front, and returns by the side flues. The latter is perhaps the one to be preferred, as the bottom or horizontal part of the boiler is exposed to the gases at the time when they are hottest, and the circulation of the water is increased by the more rapid generation of steam from the bottom. But seeing that the gases will, by natural law, always take the shortest cut to the chimney, it is necessary, in this case, to unite the side flues into one, in order that both may be utilized. If there is not length for this, it is better to take the draught the other way, namely, split it at the back and return it under the bottom, by which arrangement the side flues will pull equally.

But whichever way it is done, the *splitting* of the draught at all is an objection, as I shall show later on. Another objection to this form of setting is, that the boiler rests on two brick walls. A certain portion of the surface is thus rendered useless, and this in the best position, while any leakage from the top, or damp from the ground, may be caught by these walls, and unseen corrosion may go on.

But the chief objection lies in the smallness of area in the flues. It is impossible to get a good draught, and therefore good combustion, when the flues are small. The gases are so retarded by friction and want of space for expansion, that the entrance of air into the furnace is checked and the fire burns dead. To remedy this, a huge chimney has to be built, much higher than is at all necessary, in order to pull the gases through; the result of which is, they have not time to part with their heat in their passage through the flues, and a large portion of it escapes up the chimney. In addition to this, a large deposit of soot always takes place in these small flues, which coats the surface of the boiler and still further reduces the area and impedes the draught. Inspection is rendered difficult, as some of us, who have had to worm and screw ourselves through the flues of a small or medium sized boiler, know by sad experience; and it will no doubt be admitted that in such cases the inspection is only half done, and the boiler is worked at great risk.

Referring now to the other form of setting, viz., the *wheel* draught. I do not find these objections existing, at any rate to the same extent. The first, splitting the draught, or the unequal pull, is entirely done away with, as the gases have an uninterrupted passage from one side to the other and to the chimney. The side flues are a little larger, and inspection is easier. The chief objection to this type of setting, which has usually been sufficient to condemn it, is that the boiler is set upon a centre wall, or midfeather as it is called; and corrosion may easily be set up, and go on undiscovered.

In my opinion, the condemnation of this style of setting (that is the wheel draught), on this account only, is most unreasonable; as it seems to me to be almost as bad to set a boiler on two 4½-inch walls as on one 9-inch, especially as the position of the two walls, being so nearly at the bottom of the shell, is practically as bad for catching damp and leakages as if they were quite at the bottom.

Not that I advocate the midfeather, as usually constructed, on the contrary, I condemn it; but I find that the wheel draught lends itself most readily to a system of setting, more perfect and free from objections than any I have yet seen or heard of (and which I will now proceed to describe)—a system that has proved itself in hundreds of instances successful in the economical generation of steam, because it is based on scientific principles and follows natural laws.

This is called "Livet's system," from having been invented and perfected by M. Livet, a French gentleman who has combined a thorough knowledge of the laws of combustion, and those which govern gaseous fluids, with a practical application of the same to the generation of steam. I am indebted to him for the drawings I have here, and for much of the information I am able to give you to-night.

[The author here described the mode of setting from diagrams, specially drawing attention to the fact that the boiler is carried on iron stools, with a dividing wall only touching and not supporting it, thus securing all the advantages of the wheel draught without its disadvantages.]

The principle adopted, is that of successive expanding areas. When air or other fluid passes through an orifice forming the inlet to a gradually expanding tube or funnel, its velocity through the *orifice* will be increased by reason of its meeting with less resistance, and it will pass through the *tube* with a constantly diminishing velocity, because of the increase of area, which velocity will, of course, be still further diminished when the fluid is of such a nature as to contract in its passage, because of its taking a longer time to fill up the tube.

On this principle M. Livet constructs his flues. The back combustion-chamber is larger in area than the flue tube or tubes—the first side flue is also larger than the flue tubes, the cross section of the front chamber is larger than the first side flue, and the second side flue is larger again than the front chamber. Thus these several parts of the flues, each being of larger area than its predecessor, make up a continuously expanding whole, the proportions of each part being carefully calculated according to the dimensions of the boiler, the area of the grate, and the work to be done.

The retardation of the gases due to this cause of course allows more time for the heat to pass into the water while the initial velocity over the bridge, or the draught, is intensified by the diminution of resistance to the gases in the expanding flues.

These two results are of the greatest importance in economical steam generation, and it is impossible to obtain both of them together in any other way. A quick draught may be obtained by a tall chimney or by a forced blast, but if the flues do not act in the way now indicated, the gases will be pulled or driven through them too quickly to allow of their parting with their heat, which will thus be wasted up the chimney.

On the other hand, a slow movement of the gases through the flues may be obtained by partially closing the damper; but the result of this will only be to check the admission of air to the furnace, and so spoil the combustion. This choking or snuffing-out process actually figured some years ago in a report addressed to the Liverpool Corporation, as one of the recommendations for the prevention of smoke!

Now, seeing we all recognise and insist upon the necessity of a good draught to ensure good combustion, it may help us to realize this necessity better, if we consider for a moment what the process of combustion is, and how the draught affects it.

Perfect combustion in a furnace (I use the word "perfect" in a comparative, not absolute sense) requires a supply of air amounting to about eighteen times the volume of the gases evolved, or, in other words, about 230 cubic feet of air is required for every lb. of coal consumed; and according to the law of the expansion of gases, the products of combustion are increased in the furnace to about three times their original bulk. Hence the necessity for removing every obstacle to the passing away of the products, in order that the requisite quantity of air may be admitted.

Then, as to the process. According to Rankine, the carbon of the fuel, which forms at least 80 per cent. of its weight, passes during combustion into the gaseous state, combining with the oxygen in the air to form two distinct gases, according to the proportion in which the combination is effected. When the combustion is perfect, 1 lb. of carbon will combine with $2\frac{2}{3}$ lbs. of oxygen, and form carbonic acid gas (CO_2); the quantity of heat produced by this combination being found by experiment to be 14,500 thermal units.* (The thermal unit is the quantity of heat that will raise 1 lb. of water 1° at its greatest density, viz. a temperature of 39°.) When the combustion is imperfect, because the supply of air is insufficient, 1 lb. of carbon will combine with but $1\frac{1}{3}$ lb. of oxygen, and form $2\frac{1}{3}$ lbs. carbonic oxide (CO); the quantity of heat produced by this combination being 4,400 units only, or equal to an evaporative power of but $4\frac{1}{2}$ lbs. of water (less than one-third that of carbonic acid). When more air is supplied after the formation of carbonic oxide, the $2\frac{1}{3}$ lbs. of this gas combines with $1\frac{1}{3}$ lb. of oxygen and forms $3\frac{2}{3}$ lbs. of carbonic acid, and produces heat units equal to the difference between those named—viz. 10,100, or $10\frac{1}{2}$ lbs. water.

Now the actual process going on in a furnace is this (and for convenience of illustration I take 1lb. of carbon as representing all the fuel, and exclude the hydrogen and hydrocarbons as the quantity is but small).

At first 1 lb. of solid carbon combines with its equivalent $2\frac{2}{3}$ lbs. of oxygen, and makes $3\frac{2}{3}$ lbs. of carbonic acid gas, producing 14,500 units of heat as stated.

$$1C + 2\tfrac{2}{3}O = 3\tfrac{2}{3}CO_2 = 14{,}500$$

When air can get freely to the carbon in sufficient quantity, this is practically the whole of the process; but when this is not the case, owing to the thickness of fuel on the bars, the carbon becomes heated and expanded into gas by the hot carbonic acid in the furnace, this latter gas parting with one of its equivalents of oxygen to combine with another pound of carbon. Thus we have $3\frac{2}{3}$lbs. carbonic acid taking up an additional pound of carbon and resolving the whole into $4\frac{2}{3}$ lbs. of carbonic oxide.

$$1C + 3\tfrac{2}{3}\ CO_2 = 4\tfrac{2}{3}\ CO = 8800 \text{ units.}$$

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* Equal to an evaporative power of 15 lbs. of water from 212°.

By this second combination the *volume* of the gas is doubled and a large amount of heat is lost in the work of expansion, the heat falls from 14,500 units for the first lb. of carbon to the amount due to the imperfect combustion of 2 lbs. carbon, viz. $4{,}400 \times 2 = 8{,}800$, showing a loss of 5,700 units of sensible heat, besides the loss of 14,500 which ought to have been obtained from the complete combustion of the second pound of carbon. Should the furnace be ill supplied with air and the process stop here, this is the enormous waste constantly going on, to say nothing of the loss due to the escape of a large quantity of unconsumed carbon in the form of smoke.

If, however, air is plentiful, even should this second combination take place, the continuation of the process will be this.

The $4\frac{2}{3}$ lbs. of carbonic oxide, containing 2 lbs. of carbon, will combine with $2\frac{2}{3}$ lbs. oxygen and form $7\frac{1}{3}$ lbs. carbonic acid, giving the additional heat due to the combustion of $4\frac{2}{3}$ lbs. of carbonic oxide; that is to say—

$$4\tfrac{2}{3}CO + 2\tfrac{2}{3}O = 7\tfrac{1}{3}CO_2 = 20{,}200$$

to which, if we add the heat produced by the imperfect combustion of 2 lbs. carbon, viz. 8,800, we obtain that due to the complete combustion of 2 lbs. carbon, or

$$14{,}500 \times 2 = 29{,}000.$$

These facts show the importance of getting a *sufficient* supply of air into the furnace, seeing that by enabling each pound of coal to combine with its proper equivalent of oxygen, more than three times the quantity of heat is generated than when the supply is limited; and of course the evaporation is correspondingly rapid. But it must be borne in mind that the supply of air may be *overdone*. Air is not composed of oxygen only; but three-fourths of it by weight is nitrogen, an incombustible gas that passes unchanged as to its constitution through the furnace. This gas has to be heated up to the temperature of the furnace by the combustible gases; and the difference between the temperature of the escaping gases after passing the flues and the temperature at which the air enters the furnace, shows the loss of heat from this cause.

Assuming that air enters at 60° and escapes at 600°, the loss is 540°, which multiplied by 18, the weight required for 1 lb. of coal, and by ·23, the specific heat of air = 2,235 thermal units which have to be deducted from 14,500 = 12,265 units available for making steam. Two things thus are seen to be important—(1) that the temperature of the escaping gases should be as low as possible, and (2) that while a quick draught is essential to good combustion, there should be the means of regulating it, so that the temperature of the furnace should not be lowered by too great an excess of air. Both of these conditions are obtained in the setting before us.

1. The temperature of the escaping gases is found in all cases where the setting is properly done to be very little more than the temperature of the water—namely, from 300° to 400°, of which there is abundant and reliable evidence.

2. The damper, which is one of the features of this setting, allows of very accurate regulation of the draught. Opening back as it does in the direction of the flow of the gases, instead of vertically as ordinary dampers do, it checks the rush of air into the furnace, without offering unnecessary resistance to the gases; and an intelligent fireman, having the handle close to his fire door, may regulate the draught to the ever-varying conditions of the furnace. Some men are found to be careful enough to partially close the damper when the furnace door is open for firing, a practice which, by preventing a rush of cold air into the

flues, must be beneficial both to the generation of steam and the duration of the boiler.

I come now to notice the experiments that have been made from time to time to test the accuracy of the theoretical results with this improved setting, and (1) notice those for ascertaining the composition of the products of combustion.

At Messrs. Tates' sugar refineries in Liverpool it was found that the proportion was as follows:—

Livet's System.		Ordinary System.	
Carbonic Acid Gas .	12·17 per cent.	Carbonic Acid Gas .	7·62 per cent.
Oxygen Gas . . .	6·17 ,,	Oxygen Gas . . .	11·06 ,,
Carbonic Oxide Gas	0·23 ,,	Carbonic Oxide Gas	0·74 ,,

From which we see that in Livet's setting the carbonic oxide was to the acid as 1 to 53, while in the ordinary setting it was as 1 to 10⅓. The respective evaporization of these boilers, which were of the ordinary Lancashire type, was as 8 to 6½, a gain of nearly 25 per cent. This shows that a much greater amount of heat was generated in Livet's furnace; and the pyrometer showed that in the damper flue the temperature was under 500°, while before the boilers were reset it had been at the same place over 900°. In other cases, where the firing was not so intense as it always is in sugar refineries, the temperature of the flue 50 ft. from the furnace has been found to be 300° and under—in fact, equal to the temperature of the water in the boiler. The obvious inference from this is, that a very large amount of heat has passed into the boiler, and less waste has taken place from the absorption of heat by the nitrogen of the air. And I am assured, as a matter of fact, that the commercial results in all cases show, in increase of steaming power and reduction of fuel burnt, a saving of 15 to 20 per cent.

Another series of exhaustive trials was made by Mr. D. K. Clark, C.E. for the Smoke Abatement Committee, to test the evaporative values of different kinds of coal in a boiler set on Livet's system, at the printing works of Messrs. Clay & Sons, London. These trials have been published, and show that from Aberdare Rhondda coal an evaporation of 12¼ lbs. of water per lb. of coal was realized, the feed entering at 60°, the steam pressure being 61 lbs., and the temperature of the escaping products being 310°. From other kinds of bituminous coal from 11 to 12 lbs. was obtained during the three and a half months over which the trials lasted. The greatest care was taken to prevent priming, and the steam was frequently tested, and always found remarkably dry. Seeing that the utmost theoretical evaporative efficiency of this Rhondda coal, when tested in a laboratory, is about 15 lbs. of water, the above result may be taken as a remarkable one, and, of course, was not reached without very careful firing.

Comparative tests show a similar result. A large firm of sugar boilers in China state, that in fourteen boilers, *re*-set on Livet's system from the ordinary Lancashire plan, their consumption has been reduced from 104 lbs. of good coal per picul of sugar, to 78 lbs. of inferior coal, with an increase in the productive capability of the factory of 33 per cent. Another large firm with seventeen boilers, set on Livet's system, out of 37, state that they did ⅕th more business in a given time, and spent £2,000 less in fuel, than with the old setting. Messrs. H. Tate & Sons, before referred to, do the same amount of work with nine boilers on Livet's system, that they did before with ten similar boilers and a large economizer. I have myself, for more than two years, had to do with a Lancashire boiler, which I originally set on this system. It is 25 ft. long, 6 ft. 6 in. diam., 2 flues each 2 ft. 6 in. diam., crossed by galloway tubes. The pressure is 60 to 65 lbs., and the ordinary duty obtained, without forcing, from Somerset slack, in a compound engine with no expansion valve and unjacketed, is 116 I.H.P. I am quite sure this boiler would not give anything like such a result if set in the ordinary way.

One other important advantage of the system is, that economizers may be dispensed with. They present a great obstacle in the way of the draught, and, to be of any use, the temperature of the gases must be kept very high in the flue after leaving the boiler. In the Manchester Exhibition the feed-water was raised 180° by an economizer, and the temperature of the issuing gases was reduced 300° in raising it. As they would not be likely to enter the chimney at less than 300°, they must have issued from the boiler flues at not less than 600°. It must be, therefore, much better to get the heat direct into the water first-hand, than to let it pass the boiler, and then put in a most expensive arrangement of pipes, with automatic scrapers and driving shafts, etc., in order to collect and ultilize that which ought never to have escaped. All such contrivances require additional chimney height to pull the gases through, while with the largest boilers on Livet's system, a chimney of seventy feet in height is ample. Many 30 ft. boilers are working with the best results with only sixty feet chimneys. This is not one of the least of the advantages claimed for this system, as many firms can testify who have had sufficient confidence in M. Livet's assurances to put them to the proof, and who have found it was just as well to keep £500 or £1,000 in their bank as to spend it upon useless bricks and mortar.

The facility for inspection which is given is another immense advantage of this system. I have alluded to the difficulty there is in getting about in ordinary flues; and M. Livet has received letters of commendation from nearly all the Boiler Insurance Companies' engineers, one of which only I will quote. It is from the Chief Engineer of the Yorkshire Boiler Insurance Company, who says: "Your system is the very best on the score of inspection; the present mode of setting does not give a chance of a proper thorough inspection, except when attended with great difficulty. I can only say, that if wishing could effect such a change, I would at once have all the 3,000 boilers under my care set upon your principle, and I am sure my staff of inspectors would join me in this matter."

In the arrangement proposed no part of the boiler is hidden from view; and as the dividing wall does not in any way support the boiler, any of the half-round bricks in contact with it may be removed at will. It is also found that the shell plates remain cleaner than in the ordinary setting. The grit is chiefly deposited in the chambers at the back and front of the boiler, while, owing to the perfect combustion of the coal, very little soot passes into the flues; and not only are the plates cleaner, but smoke almost entirely disappears.

I have thus dwelt somewhat at length upon the special features and advantages of this form of setting, because I have wished to show how important a part the flues may and ought to be made to play in economical steam generation; and I think I have shown that this setting fulfils the four conditions, mentioned at the outset, by which the object of boiler setting is best attained. A legion of inventors have aimed at making improvements in the furnace, with more or less success; the form of fire-bar, the air spaces, the furnace door, the bridge, the admission of air above the fuel or behind the bridge, its exact regulation to the requirements of the fuel, and many other such points, have received their full amount of attention, and in some cases have been regarded and proclaimed by their enthusiastic advocates as the best and only remedy for defective steam generation. But, with this one exception, I am not aware that any improvements of any consequence have been made in the setting and flues of Cornish and Lancashire boilers since the day when the existing type was first introduced; and I shall be glad if the reading of this paper, to which you have been good enough to listen, may serve to attract more attention to the proper proportion and construction of the flues and chimneys, without which other improvements, however good they may be, will fall short of accomplishing that for which they were designed.

Tunnelling through various Strata.

By JOEL LEAN, Assoc. M. Inst. C.E.

Read Dec. 20th, 1887.

TUNNELLING, at the present day, is chiefly associated in the minds of people in connection with railway construction; but railway engineers cannot take to themselves the credit of even having developed the art of tunnelling. When the first railways were built, the engineers went in largely for tunnelling, for instance Kilsby Tunnel, on the L. & N. W. Railway, Box Tunnel, on the G.W. Railway, and various others, as sharp curves and steep gradients in those days were not considered workable by locomotive engines, and therefore the necessity arose of going through, instead of over or around, the hilly obstacles. But they had the help of the experience gained in canal tunnelling, and most of the men employed in tunnelling in those days had worked on the construction of the old canal tunnels; and to the canal engineers must be given the credit of being the real pioneers of tunnelling, at any rate in recent times.

The author purposes giving examples of tunnels constructed through soft, or heavy ground, as it is called;

medium ground, being neither hard nor soft; and hard ground or rock.

What is called soft ground is by far the most difficult, and requires the most care on the part of the constructors. Under this head may be classed running sand, heavy clays and faulty ground in the coal measures.

In tunnelling through very heavy ground, it is no doubt undesirable to drive a bottom heading, owing to the difficulty in supporting the ground and the very heavy timbering required, it being necessary, in some strata, to close pole the heading all round; and where a heading has to stand exposed to the air for any length of time, there is a tendency in the ground to swell, and perhaps disturb the ground above, making it a very difficult and costly matter to afterwards take out the lengths of full-sized tunnel.

The author remembers a tunnel, with which he was connected *after* the occurrence, which ran in to the surface, letting in part of a farmyard and buildings, owing to the constructors having lost their reckoning; and in endeavouring, by driving side headings, to find their way to a shaft which had been sunk on the centre line, and which they failed to meet. This multiplicity of headings was the cause of the mishap, owing to weakening the ground through too many headings, and perhaps bad timbering in addition. It was extremely difficult to tunnel through this portion afterwards; the whole of the ground seemed full of timbers of all sizes, and we came upon headings in most extraordinary places.

In cases where much water may be expected, a bottom heading is no doubt very desirable, as it so completely drains all the workings of the tunnel; but even in such cases the author does not advocate it in such strata as heavy clays, which swell on exposure to the air, as the cost of pumping is more than counterbalanced by the extra weight of the ground and difficulty in timbering. The safest and cheapest mode of tunnelling such ground is, where the tunnel is too long for time to admit of working from both ends only, to sink shafts, and then take out the full-sized tunnel in suitable lengths, care being taken to follow up as closely as possible with the brickwork or masonry lining, a sufficient length of top heading only being driven to admit of the drawing of the crown bars. It may be contended that, in some instances, it is not possible to sink shafts, owing to the height of the hill (causing great expense), or from other causes. The author does not consider, however, that such cases are often met with, as, where the height of the hill above the line of the tunnel is very great, it is not often that such heavy ground as that to which he refers is met with in any but short lengths, and he would then advocate the taking out, if possible, and lining of the lengths of soft ground, as they are met with in driving the bottom heading, if materials for doing so can by any means, without great expense, be brought to the place. Great trouble and expense would often be saved if this were done, especially through faulty ground in the coal measures, as, although these strata are not necessarily bad tunnelling ground, and are often found to be very good, fissures and faults are often met with of considerable extent, filled with soft rubbish, which, when the heading is first driven through, does not show any signs of difficulty, but which, after exposure to the air for any length of time, swells and makes a way for considerable bodies of water, which may have lodged in some basin above, and which the filling in the fissure is often able to retain when undisturbed, but afterwards gives great trouble in taking out the full-sized tunnel. The dryness or wetness of a tunnel is often the result of the ground above being undisturbed or not, and lengths of tunnel which, when taken out, have been perfectly dry, have afterwards been very wet through water having found its way from above, through cracks caused by the swelling of the ground, owing to carelessness in tunnelling.

A very important point, in taking out lengths of tunnel in heavy ground, is to place the crown bars high enough above the crown of the arch, as the bars invariably come down somewhat, owing to the great weight compressing the props and sending the footblocks, on which they stand, into the soft ground below. When sufficient allowance is not made for this, it generally necessitates what is termed poling back, that is, removing the poling boards one or two at a time, excavating the ground to a sufficient height, and then replacing the boards. This often leads to a disturbance of the ground, thus throwing additional weight on the timbering; and where the ground has already swelled before the poling back, it renders it a dangerous operation, and one which has caused the collapse of many a length, especially as it must be remembered that the whole weight of the ground has to be supported by propping off the centres; and where these are not strong enough to bear it, they become distorted, and distort the portion of the arch already turned. It should, therefore, be the first care in soft ground to have the crown bars placed *well up*, not being afraid of their being a little high, which is a good fault rather than otherwise. The centres used should also be of ample strength, and then the miners may consider themselves prepared for any emergency.

In the coal measures, dangerous ground is often found where thin beds of shale, coal and fireclay alternate. The coal is in most of these cases much thinner than either the shale or the clay; and where the strata is dipping at all steeply, the shale and coal often slip off the fireclay, especially where a spring of water occurs, making the clay wet and slippery. Caution should be exercised in cases where these occur, as the ground, when excavated, often looks very sound and strong, and, from appearance only, would lead one to think that it would stand without timbering, especially in the sides of the tunnel; but narrow escapes have often taken place from this cause, one or two of which have come under the author's observation, and which he has witnessed as a rather too near spectator. In ground of this kind, the timbering should be carried at least below the springing of the arch, and not, as is too often the case, merely round the crown and haunches of the arch. The ground which is referred to is more often met with in tunnels through the coal measures, chiefly through thick beds of Clift and Pennant rock, which require little or no timbering, and, therefore, there is often a tendency to risk a length or two of these beds with insufficient timbering.

There is no doubt that the reason of so many of the distorted and sunken arches in many tunnels,—and some of them are very bad,—is owing to the want of a little foresight, and to the careless way in which the timbering has been done, very often by men of experience. You will often find that practical miners, used to timbering heavy ground, will over and over again make the same mistakes, and will make the tunnel too large or too small in exactly the same places, although admonished time after time.

Tunnels are nearly always taken out too small at the crown, and too large at the haunches, the direct opposite of which would be the better way; but it is almost impossible to make miners alter their ways in this respect.

One of the chief causes,—in fact, almost the only cause,—of so many tunnel arches being flat at the crown, is from the preceding reason. The tunnel has been taken out too small at the crown, therefore a ring or two of brickwork is left out, to pass the crown bars without going to the trouble and expense of poling back, thus weakening the crown where most of the weight occurs; the spaces behind the brickwork in the haunches are left unfilled, and in a very short time after the centres are struck, the arch takes the distorted and dangerous form which is so often seen. Indeed, the wonder is that so many of the tunnels, constructed when the supervision was lax in former days, have stood like they have. Experience is a grand thing, but it is not everything, and often seems to confirm some men only in the old rule-of-thumb ways learnt in their youth, and which it is almost impossible to eradicate. What is wanted is more *intelligent* supervision, and not merely *rule of thumb.*

A source of great danger exists in tunnelling through the coal measures near collieries where the ground has been undermined by the workings. Very often coal is prospected for and headings driven which are afterwards abandoned, and no record left of their position, or any plans of the workings. So in tunnelling through the neighbourhood of many abandoned coal mines, it is often impossible to tell where to expect these old workings. And they may be met with at most unexpected times; and, being often full of water, are often a source of great danger and expense, in addition to the ground in their vicinity having been disturbed by the rotting of the timber with which they were supported, or in many cases by the timber having been withdrawn for other purposes, and the workings left to their fate. A short tunnel on the Penaar Branch of the Great Western Railway, which was constructed a short time ago, although only about 220 yards in length, was found to be one mass of old workings, nearly from end to end; and what was expected to have been a tunnel through sound Pennant rock, and of a very simple nature in its construction, turned out a very expensive piece of work, and required the most careful and costly tunnelling. In some places there were great holes, some 10 to 20 feet in height above the top of the tunnel, filled with rubbish; and these holes had to be most carefully timbered, and closely packed with masonry or stone, owing to the ground having been so disturbed in their neighbourhood by long exposure and neglect. This nearly doubled the expected cost of the tunnel. These old workings were no doubt not entirely unknown in the neighbourhood; and probably, if the engineers had made inquiries, they might have formed some idea as to their nature and extent; and so, what was expected to have been a simple little tunnel would not have turned out a very troublesome and costly one; but the extent of the difficulty would have been known from the first and arrangements made accordingly. This shows the necessity in the coal measures, anywhere in the vicinity of collieries or old workings, of first obtaining as thorough a knowledge as possible of the position of these, so that they may be avoided if possible; or, if this cannot be done, that proper precautions may be taken in the first place, and the exact nature of the difficulty and the cost known without its coming afterwards unexpectedly in the shape of a large bill for extras from the contractor for the undertaking.

Fortunately, owing to the tunnel referred to being so short, there were no shafts, and it was entirely worked by bottom headings driven from both ends, so that the water found blocked up in the old workings ran away by gravitation; otherwise, if it had been met with in shaft workings, it would have necessitated a considerable outlay in pumps, or a delay of the works until the headings from the ends had been driven of sufficient length to tap the water; but this might easily have occurred in a tunnel of greater length, and would have been the more inexcusable, as it might have been avoided by a little care and foresight.

An instance of the collapse of a tunnel owing to being undermined by colliery workings is the case of Hirwain Tunnel, near Merthyr, on the Great Western Railway. This tunnel had for years given great trouble, and bricks were constantly falling out of the crown of the arch, and repairs were on the same scale diligently proceeded with, until at last the tunnel entirely collapsed for a length of about 100 yards, and completely stopped all traffic for the space of about twelve months. It was supposed that the bricks in the crown of the arch had crushed, and an inside lining had been in progress for a considerable time, there having been ample room owing to the tunnel having been originally built for broad gauge.

The method adopted for making good the collapsed portions was timbering down from the surface, taking out the damaged work, and re-building with brickwork in cement. The timbering required was very massive, and probably amongst the most extensive of the kind that has ever been carried out; but it saved immensely in point of time, besides being able to thoroughly examine the damaged portions, and rebuild in a stronger and more thorough manner than could otherwise have been done.

The tunnel had been giving trouble for years; but it was not suspected that the colliery workings were the cause of the mischief, and it was hardly thought that they extended under the site of the tunnel. Afterwards surveys of the workings in the vicinity were obtained, and found to extend very considerably under the collapsed portion of the tunnel.

This shows the necessity, as was said before, when tunnelling through colliery districts, of engineers making themselves thoroughly acquainted with all the workings in the district, so as, if possible, to avoid them or to take precautionary measures for the future safety of the tunnel. Railway Companies now usually purchase, if possible, the mineral rights in colliery districts; but it was not so formerly, and hence the great trouble and expense they have been put to in many instances, not only with tunnels, but also with open portions of their lines; but, of course, in no case is subsidence so disastrous as in that of tunnels.

About Wolverhampton, and through the black country, the bridges are built purposely with the view of subsidence taking place, and the girders rest on wedges which can from time to time be tightened; and when the girders have been lifted to the full extent of the wedges, the abutments can be raised with brickwork or masonry as the case may be, and the wedges again placed in position for future slight raisings. Of course this cannot be done in the case of tunnels, as they cannot be built on wedges; and therefore every precaution should be taken to secure their permanent stability. The author believes that formerly in the black country sudden depressions of the permanent way, of one or two feet, would take place, which had to be instantly made good with ballast in order to keep the traffic going; but whether such occur to the same extent now, he cannot say. An instance of tunnelling through the coal measures is Swansea Tunnel on the South Wales Railway of the Great Western system. Brunel, who was the engineer, had at that time a most unaccountable liking for flat-crowned arches in his tunnels; and it is all the more unaccountable considering his great practical ability and acumen. This tunnel is through a blue, sandy clay, very wet and very heavy. The author remembers his father, who was an assistant of Brunel's on that very work, saying that one night (Brunel had the habit of turning up on the work at odd times, and unlike any other man) a messenger rode up to his house post haste, to say that Brunel was in the tunnel, and that he had better come down at once. He hurried off to the tunnel, and, as the shortest way of getting down, slid down the rope of the shaft. He found Brunel there, who made a most thorough examination of the tunnel, as it had been constantly reported to him that the arch was too flat. The arch in many places was as flat as the ceiling of a room at the crown, having settled tremendously. Brunel asked what the author's father could suggest as a remedy, and he merely said, Give the arch a foot or two more rise. Brunel, who never, if he could help it, altered his designs, said he could not hear of it, and soon afterwards went away; but a day or two later down came an alteration of the drawing, showing an arch with greater rise. Why Brunel should at that time have taken a fancy to flat arches in tunnels, it is impossible to say, as it is almost the only place where they are undoubtedly out of place, especially in heavy ground. There are one or two more things unaccountable in Brunel, such as his altering from the extreme of costly masonry, as on the Bristol and Paddington Railway, to the very roughest of random work; and in his later years he would not have any dressing of the stone on most of his railway bridges. Doubtless the latter served its purpose, but still it was going from one extreme to the other; and the author has seen the extreme of bad masonry in some of Brunel's later railway work; but, doubtless, like most great men, he was not infallible. It was almost impossible to get him to alter a design after the work was once begun; and if a bridge fell down, he would build it again in exactly the same way. Doubtless, he was often right, as a bridge would often fail through bad work or the design not having been properly carried out; but, still, perhaps a *little* more pliability would have corrected what appeared to his greatest admirers one of the chief faults in his character.

The next portion of the author's subject he purposes dealing with is, tunnelling through medium ground—that is, ground which does not come under the head of being very heavy, and requiring very heavy timbering, nor yet is it any more fit to be classed as rock tunnelling. Such ground as this would be chiefly through the better portions of the coal measures, hard and compact sands, as are found in the Oolitic formation, through the chalk, and, in fact, any ground which does not require a great amount of blasting, or, on the other hand, heavy timbering. In such cases as these, a bottom heading is, no doubt, the best form of beginning operations, as these kinds of tunnels are not necessarily free from water, and therefore the bottom heading is required for drainage purposes, if for no other. It is also easier to form break-ups for the purpose of enlarging to the full-sized tunnel; and as the ground will in most cases stand with little or no timber, it is the cheapest and most expeditious mode of carrying out the work; and after the heading is sufficiently advanced, any number of break-ups can be formed, so as to expedite the work to the required degree. To form a "break-up" is usually done by breaking down over the roof of the heading, until sufficient space has been made to erect a scaffolding, which then receives the dirt or rock without blocking up the heading, and leaves it free for the passage of wagons and materials from other parts of the tunnel, or from other break-ups. The breaking down on to staging can, where the ground will stand, then be carried forward for a long distance, without arching the lengths as you go along, and saves a great deal of time. It is usual for the ground to be broken down for about the width of the heading right up to the top of the tunnel, as ground which would not stand if widened out at the sides, will often stand in this way for any length of time, and it serves to expedite the progress of the work. Walking along the bottom heading whilst this work is in progress, a person unacquainted with the work would think that nothing but the heading had been done; and yet, at the same time, nearly two-thirds of the tunnel excavation may have been completed, and, therefore, in measuring up work for payment on account, a good practical knowledge is really required to tell the real value of the work and to estimate its true value. A few days afterwards, perhaps, the staging may be taken down, and then the real extent of the progress is visible, and to any one not in the secret, would seem magical, as from the mere heading the tunnel would have been suddenly transformed into nearly its full size. It has often seemed to the author that the remarks of the uninitiated are not always so foolish as they appear, when one sometimes overhears them; and that the apparent change from chaos to perfect order must appear rather wonderful to those persons. One method of tunnelling ground of this nature, is to take out the whole arch of the tunnel in the manner before mentioned, and then turn in brickwork or masonry, as the case may be. Afterwards, when the lower part of the excavation is completed, underpinning the arch, and thus completing the side walls; but the author does not like this mode, as, although it is without doubt very expeditious, he does not consider that the underpinning can be done so as to ensure the wall being as sound as if built up in the usual way, and his own experience has led him to doubt that the work is usually done in a proper manner without the closest supervision, which it is very difficult to give in person, or to be certain of obtaining it from others; and he always feels inclined to discourage any method of working which is difficult of supervision, and of the soundness of which there is never any satisfactory conviction. An instance of a tunnel being completed in a remarkably short space of time, is the case of the Combe Down Tunnel, on the line between Bath and Bournemouth, about two miles from Bath. This tunnel is through sand of the Inferior Oolite, with beds of rock, and underlying the Fullers Earth. The site of this tunnel was extremely well chosen, as it was nearly dry throughout its entire length, and in constructing the tunnel it was found that it lay between two large water basins, as in places it appeared in the bottom and in others at the top of the tunnel, and slight alterations were made in the gradient, so as to avoid the water both at the top and bottom of the tunnel. When this tunnel was first projected, it was thought that it would turn out to be unusually wet, as large streams of water issue from the hill, on the Midford side of Combe Down especially, but at a much higher level than that of the tunnel; and it was considered likely that some of this water might find its way into the tunnel.

The length of the tunnel is about 1,900 yards, and a bottom heading was driven throughout its entire length, from both ends, and the tunnel completed without the aid of shafts, within eighteen months from the commencement. The sand was of a hard and compact nature, and would stand in most places with little or no timbering, being easily excavated with a pick, and could, in fact, be cut out with a penknife, although powder was used, as it was found cheaper to blow it out in large masses. A considerable length of this tunnel was not lined, especially where a rock top was obtained, and very little had sidewalls. Where lining was required, it was in nearly every case only in the form of a flat fly arch springing off the rock or hard sand-sides of the tunnel. In many instances as much as 22 yards of heading per week was driven, by hand labour, through this compact sand. The author considers that there are not many instances of tunnels being completed in so short a time, as it must be remembered that, as was before stated, the tunnel was over 1,900 yards in length, and constructed entirely without shafts, the only shafts being at the ends of the tunnel, and sunk chiefly for lowering materials into the tunnel without obstructing the cuttings, no spoil being, in either case, wound up the shafts, nor were they used in any way for driving the tunnel. Of course it must also be borne in mind that the ground through which the tunnel was driven was unusually favourable, and in very few instances would such favourable conditions be likely to arise. If the author remembers correctly, the tunnel was straight from the Bath end, and on a curve of 40 chains radius at the Midford end, the Bath end being nearly level, and the Midford end for a considerable distance on a gradient of 1 in 50.

It was, no doubt, an error to suppose that this tunnel would turn out very wet on account of the large streams of water issuing from the hill not far from its summit; and it was overlooked that between the proposed tunnel and the water lay the Fullers Earth, which is in itself impervious to water, and a great water-basin, from which, no doubt, the streams of water issued. Most water was met with in the bottom of the tunnel, and hardly any from above; and the only water found was in the rocky portions, the hard sand being almost uniformly dry throughout its whole extent. Although such a large proportion of the tunnel was left unlined, and passed by the Board of Trade Inspector in that state, the writer believes that since the line was opened, and especially since it has been worked as a joint line of the Midland and South Western Railways, a large portion of it has been lined; before that was done, large falls of sand occurred, and it was most unwise to leave such a tunnel in a naked state, it being done, however, on the score of economy, to which account, we fear, must be laid many subsequent large outlays on most of the railways in the kingdom, many narrow escapes from serious accidents, and probably many serious accidents themselves.

Tunnels should, where possible, have a rising gradient from both ends, with the apex somewhere near the centre, as it so greatly assists the drainage and construction, and where the tunnel is driven from the ends only, and much water is met with, the saving in pumping is very considerable. No doubt in many cases this cannot be done, especially where a tunnel is situated on a gradient of any length and steepness, but where the gradient is slight it can, in most instances, be arranged so as to approach the centre of the tunnel with a rise from both ends. In addition to the extra difficulty and cost of constructing a long tunnel on a single steep gradient, there is the disadvantage of having to pass the whole of the drainage water, from the cutting at one end, through the tunnel; and where the cutting, as in many places, is of any length, through wet ground, this generally means a large body of water. It generally means, also, a great saving in the time of construction, where there is a gradient from both ends, however slight that gradient may be; as, however much trouble and expense may be spent in pumping machinery, there is always great trouble and delay in driving down-hill, which can never be satisfactorily obviated. The author knows that it is at times practically impossible to have a gradient both ways in a tunnel, but he also knows that this is not always the case, and considers that it should be done in all cases where reasonably possible. It is, doubtless, always possible, in constructing on a down gradient, to run the tunnel as far as it will go with a slight rise, or on the level; but this means afterwards a lot of underpinning, and, as was said before, he always feels inclined to discourage that mode of working, except in cases where it is absolutely unavoidable.

The author now comes to the third portion of his subject, viz., tunnelling through hard ground or rock. Although under the two previous heads there might be some amount of rock to tunnel through, yet under the name of hard ground tunnelling, is here meant ground which cannot be excavated without blasting, and such as would be met with in the Mountain Limestone, Devonian, and chiefly amongst the older formations. The usual way of beginning operations would, no doubt, be in almost every instance by driving a bottom heading; and, as a rule, tunnels through ground of this nature rarely have shafts, owing to the hardness of the ground through which they would have to pass, and the expense of sinking; besides, in many cases, owing to their great depth, tunnels of any length in such strata being generally through hills of considerable height, and probably inaccessible for shafts. The author considers it quite an open question as to whether hand labour is not still the *cheapest* mode of drilling rock (although, of course, it cannot compare with machine drills in point of speed) when the cost of compressing the air, the wear and tear, and breakdowns, are taken into consideration, especially for widening the heading to the full size of the tunnel. His experience is—that the loss of air owing to leakage in the portable tubes, and the constant damage and wear and tear to same, necessitating also so many stoppages, more than counterbalances any other advantages gained by their use, besides the often reckless placing of the holes, and the unnecessary number of them, both wasting labour and blasting powder. It is not intended here to enter into any controversy as to the relative merits of different kinds of drills, or of machine and hand drilling, but merely as to the relative apparent advantages, comparing time taken and the expense of drilling. Time generally means money; and the cheapness of hand labour may be more than counterbalanced by the loss in time and the additional expense accruing from a work being spread over a longer space of time. The author had some experience a few years ago with the diamond drills of the Diamond Rock-boring Company, who were the contractors for a tunnel in South Wales. For heading driving this is no doubt the quickest and most expensive system of all, the amount of power required being very great, and the loss in diamonds at times enormous, owing to their falling out of their sockets. This system is not suitable for general boring, such as would be required for enlarging to the full size of the tunnel, owing to the cumbersome nature of the machinery, and was not used for such in this case, but merely for driving the heading, which he does not hesitate to say might have been driven for one-third or probably one-half less by hand labour. It is not intended to disparage the diamond drill in its own particular province of trial-boring, where it has no doubt done splendid work, but merely to give an example of its use in an ordinary tunnel heading. The machinery of the diamond drill is very pretty. It consists of a small air oscillating engine attached to a standard, which is fixed on an iron trolly frame, the diamonds being set round the head of a hollow steel tube, which is revolved by the oscillating engine, and thus cuts its way by sheer friction into the rock, the core of which passes through the hollow tube, coming out at the end in a complete state, thus forming a perfect geological section of the ground passed through, which is, of course, of immense advantage in trial-boring for Artesian wells, or similar purposes; and it has no doubt done splendid work in such cases, but is not adapted for heading driving. For driving the heading referred to, a pair of drills were fixed in the manner referred to on an iron trolly frame, and when a sufficient number of holes were driven, the trolly was run back on the ordinary rails of the dirt wagons, out of the way, until the holes were fired, when it would be again run into place for drilling. Owing to the amount of complicated machinery, it was always necessary to have a skilled fitter as foreman over the drilling; and the cumbersome machinery was nearly always in the hands of fitters, which soon rubs the gilt off of the speed whilst actually at work. It is not intended to enter into any description of the different kinds of air drills, of which the name is legion, although differing little in principle, and many improvements in which have been made since the diamond drill first came out; but the author thought that the foregoing brief description would not be out of place, as it is not often met with in heading driving. The work done in the tunnel referred to was largely through the Pennant rock, and that of the hardest of its kind, although a considerable portion lay through a hard kind of what was called Clift, 18 yards per week being often driven through it, and 15 yards through the hard Pennant. A heading through hard ground should be of ample size, within reasonable limits, say not less then seven or eight feet square, as it is much cheaper to drive a heading of sufficient dimensions than to have to widen afterwards. It is often the case that tunnel headings, through hard ground and where no timbering is required, are driven too small, under the mistaken idea of its being economical, but it is not so, as working in a cramped space, specially in rock, is always disadvantageous, especially when proceeding to enlarge to the full size of the tunnel; and when the advantages of the large heading, as well as the additional amount taken out of the full size of the tunnel by it, are taken into account, they more than balance the extra cost of driving the heading of ample size. This, of course, must be taken to mean of ample size only to leave room for proper working, as it would not pay to take out the full size of the tunnel in the shape of a heading, besides being a waste of time. It is not always waste to expend a little more money on the speed of the heading, as the faster the heading advances, the more energy and scope is left for completing the tunnel, and a slow heading always cramps the remainder of the work; therefore increasing the speed of the heading-driving is often money well spent, although the actual cost of the heading may be enhanced. A point of considerable importance in the economy of completing a tunnel through hard ground, is to exercise care in the levels, so that little or no bottoming up may be left, which is always a source of great trouble and annoyance. The author has often known headings of this kind driven in a careless way, not from wrong levels having been used, but from the miners not being properly looked after, and which generally entails a lot of expense and trouble afterwards, which might easily have been avoided. The same remarks refer in an equal or even greater degree to keeping the heading as true as possible to the centre line. The author has often, when he has remonstrated with the contractor or his men on this point, had the reply, "What does it matter, so long as the heading is not outside the tunnel." It does matter; as, if you had a heading all on one side of the tunnel, when you proceed to enlarge to the full size, it makes a very great difference whether your heading be in the centre or not, as you have all the hard driving on one side, and none on the other, therefore, your heading is not the benefit it should be, besides having to remove the excavated material all over to one side to fill it into the wagons. The author is indebted for much sound practical advice on these matters to his father, whose experience in tunnelling has been very great; and although they may appear trifling to some, yet in these days of competition and cutting down of cost, they are not to be lost sight of, and, taken together, make all the difference in the economical construction of a tunnel.

In rock tunnels, it is often unnecessary to have side walls, as the ground will generally stand well enough without, although an arch is very seldom dispensed with nowadays, although it was formerly done to a large extent. Where arches only are adopted, they should have as small a rise as is consistent with safety, so as to spring directly off of the sides of the tunnel. It is very essential in these cases to have arches of large radius, as they then do not bear any downward weight on the sides of the tunnel, and are not endangered by any after displacement if the sides should at any time require facing up with brickwork or masonry, which often happens in tunnels of this kind. It being borne in mind that in rock the sides require, if any, mere face walls, and which will not have to bear any direct thrust, but only to keep any stray lumps which may become loose from the vibration of passing trains. It is very essential in all tunnels, but perhaps more particularly so in rock, where springs of water are often met with issuing from fissures, to leave weepholes a little above the level of the rails, so as to preserve the walls from any pressure which may arise from the accumulation of water behind them; and they also tend to keep them dry, and prevent the general dampness which is so evident in many tunnels, and thus help to preserve the materials with which the tunnel is lined from decay, owing to the action of frost forming ice on the wet portions. The writer has seen tunnels of considerable length one mass of icicles from beginning to end, and it is a mistake to suppose that frost acts on them a short distance from the ends only.

Good drainage also keeps the rails dry, and free from that general greasiness which is the case in many tunnels, and, where they are on steep gradients, materially retards the tractive power of the engines.

Continuous Railway Brakes.

By ARTHUR WHARTON METCALFE,
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Read January 17th, 1888.

PART I.

THE great interest taken in the subject of railway brakes by the public generally, and by engineers in particular, and the favourable reception usually accorded to papers on this subject, have encouraged the author to bring this paper before the Engineering Section.

The early history of the railway brake is identical with that of the brakes used on the vehicles employed on the plateways and tramroads in colliery districts, and on the lines which connected these with the ports from whence the coal was shipped.

Some of what are considered the most recent and most important improvements in railway brakes can be traced to these early brakes, among these are :—

I. The use of cast-iron in brake blocks.

II. The distribution of the brake-pressure over a large surface, and the consequent reduction effected in the wear and tear of brake blocks.

These improvements are due to Mr. Le Caan, who patented them, and described them in a paper read before the Society of Arts in 1801. This brake was applied to a common cart running on rails. A description of it will be found in Mr. M. Reynolds's work on Continuous Brakes.

In Le Caan's brake the brake blocks surround about half the circumference of the wheel; in the modern brake the proportion of the circumference to be braked is equally shared by two brake-blocks, between which the wheel is gripped.

Very simple forms of brakes sufficed for the earliest railway trains; but with the growth and extension of the railway system, improved means were required for the more complete control of trains; and as the usual speed and the weight and number of trains to be managed increased rapidly, and the demand thus became urgent, inventors were stimulated to bring out many mechanisms for which they claimed increased efficiency in arresting trains.

Railway engineers and many others connected with the design and manufacture of rolling stock, when questioned on the subject, say this has been their experience; but it is not until one has had occasion oneself to search the records of railway brake inventions that one realizes properly the amount of ingenuity that has been devoted to designing railway brakes, and the great number of inventions of which it has been productive.

About 1875, the year when the Newark brake trials were undertaken by the principal English railway companies at the instigation of the Board of Trade, the stream of inventions received a natural check; not because inventive brains were exhausted, but because the conditions a perfect brake should satisfy had come to be more fully understood, and the difficulty of designing a mechanism that would comply with these conditions more perfectly realized. At the

Newark brake trials, only those brakes were tried which were the result of long and extensive experiment on the part of their inventors. These and other brakes will be described later on.

The field of competing inventors was at this time only occupied by those who had the time, money, as well as the forethought, to carry out the long series of experiments on a large scale necessary to bring such a mechanism as the continuous brake to a high degree of efficiency and perfection, as it was from such experiments alone, conducted with care and backed with capital, that a solution of the brake question could be expected. By this species of natural selection, the brakes of those inventors only who had fair claims to show of having, in a great measure, solved the brake question were placed in competition at the Newark trials.

It will be necessary here briefly to consider the conditions which must be adhered to in order that the traffic on a railway where trains follow each other frequently and at short intervals may be safely and expeditiously managed, and the punctuality of the trains ensured.

From a consideration of these conditions it will be possible to deduce the qualities which a continuous brake should possess in an essential degree, if it is to be suitable for the exigencies of modern railway traffic.

CONDITIONS CONTRIBUTING TO SAFETY.

The first condition for safety is, that the trains, which, as regards signals, are protected, should be equally so as regards the means placed in the hands of the engine-driver and guards for controlling the speed of the train between stations, on approaching junctions, or when descending steep gradients.

The second condition for safety is, that the train should be provided with contrivances of an automatic character, for instantaneously applying the brakes in the case of broken couplings or derailment of the train, any action of the driver or guards being thus anticipated.

A brake which gives the driver complete control of a train during ordinary running, and so long as it keeps the rails, is a most valuable safeguard so far as the fulfilment of the first condition for safety is concerned; but the brake must be unfailing in its action, so as to justify the driver's confidence in its use.

Many brakes, including some sectional brakes, comply with this condition; but though admirable in many respects, they do not provide for every emergency which may arise, such as derailment of the train, or the case of a train becoming parted when ascending a steep bank. To be useful under all circumstances which may arise, a brake must possess certain qualities which will enable it to comply with the second condition for safety; that is to say, in addition to being reliable while the train keeps the rails and is under the control of the train officials, it should also anticipate any action of the driver or guard in the event of derailment of the train or of its becoming parted; in short, the brake should be "automatic in its action" in case of accident.

The writer has dwelt at some length upon the conditions for safety, because it enables one more fully to realize the difficulties which had to be mastered in designing a brake that should conform with the Board of Trade requirements as regards continuous brakes. These requirements, to be quoted directly, are extensive; the standard they have set for continuous brakes is very high, but has nevertheless been attained. The standard, by being high, has contributed to the safety of travelling in a great measure; and the Board of Trade deserve the credit of having from the first taken up a strong and unassailable position upon this most important question.

The following are the "Board of Trade Requirements relating to Railway Continuous Brakes," as set forth in the circular to the railway companies, dated August 30th, 1877:—

BOARD OF TRADE REQUIREMENTS.

(*a*) The brakes to be efficient in stopping trains, instantaneous in their action, and capable of being applied without difficulty by engine-drivers or guards.

(*b*) In case of accident, to be instantaneously self-acting.

(*c*) The brakes to be put on and taken off with facility on the engine and every vehicle of a train.

(*d*) The brakes to be regularly used in daily working.

(*e*) The materials employed to be of a durable character, so as to be easily maintained and kept in order.

The first three of these "requirements" are those which bear directly upon the action of the brake mechanism; on this account the author proposes describing a number of the brake systems which are in extensive use, and others which though now discarded, have until recently been in general use. He proposes further to consider in what respect those which hold the field comply with the above requirements, and how far they may be considered as successful solutions of the brake problem, and in what respect the systems discarded have failed to meet the requirements of the Board of Trade.

PART II.

The author has now arrived at the second portion of his paper; viz., the consideration of the brakes themselves, and the classification which may be suitable for continuous brakes. The writer has before referred to the great number of brake mechanisms which have been invented. In a paper on the "*Classification of Continuous Railway Brakes*," read at the Institution of Civil Engineers, on December 2nd last, by the author, an attempt was made by him to classify continuous brake mechanisms in a systematic manner; and though the classification of *continuous* railway brakes was mainly dealt with, that of sectional brakes was likewise incidentally discussed.

The facts which led to the method of classification eventually adopted,—facts by which no one who has gone into the question of railway brakes can fail to have been struck,—are very briefly summarized, as follows:—

1. The very large number of brake mechanisms invented, including sectional and continuous brakes.

2. The limited number of different principles underlying the action of the mechanisms to be found.

3. The obvious family resemblance to be discovered between the mechanisms in a group, when classified according to the principles upon which their action was based.

4. That whatever the classification adopted may be by different writers, one line of division will be found common to all systems of classification proposed, which separates brakes which are *sectional*, or only applied to certain vans placed at front, middle, and rear of a train, at best, and more usually to front and rear van only, and *continuous* brakes, which are applied to every vehicle throughout the length of the train. Among continuous brakes, another natural line of division is suggested by the "*a*" and "*b*" conditions of the Board of Trade requirements, namely, the division of them into "simple" continuous brakes, complying with condition "*a*" of the circular; and "automatic" continuous

brakes, complying with both "*a*" and "*b*" of the requirements as regards their action.

Definition of "Brake."—If we define a brake mechanism in its widest and most general sense, we might define it to be a mechanism designed for the purpose of dispersing, absorbing, or transforming the kinetic energy of a train.

Furthermore, it will be found that, as regards the classification of—

SECTIONAL BRAKES,

they may be classified according to the manner in which the kinetic energy of the train is dispersed, and the form of instrument directly employed in its dispersion. Thus we have Block brakes, Slipper brakes, and Clip brakes; also Pump brakes, Fan brakes, and Sand brakes. It will be sufficient to indicate the method of dispersing the kinetic energy of the train in one, or two, or three of the brakes just named.

In the case of the "Block" brake, the friction set up between the wheel tires and the blocks, which are pressed against them by suitable gearing in the vans, generates a large amount of heat, in which form the kinetic energy of the train disappears.

In the case of the "Slipper" brake, by means of suitable gearing, the weight of the brake van is transferred from the wheels to a "slipper," where the previous rolling friction of the wheels on the rails is altered to a sliding one, and an increased amount of the train's energy is absorbed in overcoming the extra frictional resistance thus set up.

In the case of the "Clip" brake, by means of suitable gearing, a "clip" is made to grip the rails; and the frictional resistance set up between it and the rail, and also the molecular stresses set up in the parts of the brake mechanism, are the means of absorbing the train's kinetic energy.

In the case of "Fan" and "Pump" brakes, the fluid friction of a "fan," keyed to one of the axles and revolving in a cylinder filled with liquid, which it has likewise to circulate through a plate perforated with holes, is used to absorb the train's kinetic energy.

Details of these brakes are to be found in Rankine's works, and in Mr. J. Wolfe Barry's "Railway Appliances."

Sectional brakes are interesting and instructive in many respects, but are not of sufficient importance to call for a lengthy description. Their day is over, so far as passenger trains are concerned; and in America and other countries they are being superseded on freight trains by "continuous brakes."

Among—

"CONTINUOUS BRAKES,"

We find, as before said, two great divisions:—

A. Simple Continuous Brakes, and
B. Automatic Continuous Brakes.

Each of these divisions can be divided into the same number, namely, three classes, any class among the Automatic continuous brakes having the same distinguishing characteristic as the corresponding class among Simple continuous brakes.

Continuous Brakes may be classified according to the "force" available for working them, and the method of its application to that part of the brake mechanism where the power is applied.

Classified in this manner, we have—

Class I.,

Containing those brakes in which the force available for working them is derived from the manual power of the guard, applied to a hand-wheel in the guard's van, the power being applied by suitable gearing, with a certain mechanical advantage, to the "brake rigging," by which the blocks are brought into frictional contact with the tires of the wheels, a resistance being thus set up which serves to disperse the "accumulated work" of the train.

A second class,—

Class II.

Contains those brakes in which the "force" for working the "brakes" is the "kinetic energy" of the train, which, by means of suitable gearing, worked from a friction roller on the rear axle of the tail brake-van, is made to revolve a drum, on the shaft of which a chain is wound; the shortening of the chain raises a pulley, fitted to the end of a bell-crank lever on each carriage, under which pulley the chain is passed; inclined thrust rods, linked to the vertical arm of the bell-crank lever, transmit the motion to the levers carrying the brake blocks, which are thus applied to the tires of the wheels.

In order to apply the brakes, it is only necessary to bring the friction drum, on whose shaft the chain is coiled, into frictional contact with the friction roller on the brake-van axle, for which the device shown on the "friction rollers" of Clark's brake is provided.

It will be seen, that should the train become parted, and the chain broken, the means of applying the brake is gone, as the chain would have nothing to tighten against.

Class III.

Contains those brakes in which the force for working the brakes is obtained by destroying a certain condition of "air pressure," of "liquid pressure," or of vacuum, which, up to the instant of application of the brakes, had been maintained on both sides of a moving piston—or, in some instances, a fixed one, where a movable cylinder is carried on each vehicle, and is connected with the levers carrying the brake blocks. The destruction of the equilibrium which maintained the piston at rest and the brakes "off," is brought about by altering the above-named conditions of pressure or of vacuum on one side of the piston, whence motion of the piston or cylinder ensues—according as it is the former or latter which is movable, and connected with the brake rigging.

The part of the mechanism which transmits the available power from the point where it is applied to the point where it is utilized,—usually termed the "brake rigging,"—is much the same in all continuous brakes, and needs no particular description.

To many, the abstract just given of the "classification of continuous brakes" may appear meagré, and of too theoretical a nature; this arises from the fact that where a classification of mechanisms is based upon "principles," and not upon details of construction, the ground covered or affected by these principles cannot be exactly defined. New inventions may appear, which may be further manifestations of particular principles; therefore, unless we are to be constantly reconstructing old classifications, or devising new ones, we must state "principles" in such general terms as to afford grounds for believing that the classification of mechanisms in which they exist may be broad enough to include inventions that have been and inventions that may yet be made. The number of "principles" to be found are fewer than the number of applications of them which may be made.

What a general "classification" of mechanisms lacks, but more especially such ones as "railway brakes," of which there are a great number, may be remedied by detailed description when particular mechanisms are analysed.

The author proposes describing, so far as time will allow of, as many of the "continuous brakes" of all classes, both automatic and simple, as are interesting on account of their extensive use at the present time, or because they mark an era in the development of the continuous railway brake.

A. EXAMPLES OF SIMPLE CONTINUOUS BRAKES.

The Continuous Screw Brake.

This is an example of Class I.

The power for working the brake is applied to a hand-wheel in the guard's van, which, by means of bevel gearing, communicates a rotary motion to a shaft extending along underneath the carriages.

The revolution of the shaft screwed up the brakes on each carriage under which the shaft extended. This shaft was, of course, made in lengths, one of which was carried by each carriage of the "set" over which the continuity of the "brakes" was to be maintained; and the extremities of the shaft lengths were formed to make suitable couplings.

It is evident that, provided the "power" of the guard was sufficiently increased by the screw gearing, the "continuous screw brake" may be regarded as a good and reliable brake, and one which was not likely to get out of order.

In the event of the train breaking in two, the portion of the train with which the brake-van remained, would still possess the means of stopping.

To increase the utility of the continuous screw brake, a front and rear brake-van were usually attached to the train, by which the risk of a part of the train being left without braking power, in the case of accidental splitting of the train, was quite avoided, in addition to which the power of another man was gained. This brake is still in use on a few branch lines, among others, the Cheddar Valley line.

Clark's Chain Brake.

This is an example of a brake belonging to Class II.

A friction-roller is carried on the last axle of the brake van, which serves, by frictional contact when set up, to revolve two friction drums carried in swing links, one on each side of the van axle carrying the friction roller.

The shafts of these two friction drums carry barrels, on to which the chain used for putting on the brakes is wound.

This chain passes from the "winding barrel" *over* a pulley, carried in a swing frame, then underneath another guide pulley, carried at the extremity of the horizontal arm of a bell-crank lever, to the end of the vertical arm of which the inclined thrust rods which work the levers carrying the brake blocks are attached; from the "guide pulley" just mentioned the chain again passes over another guide pulley, hung in a swing frame like the first, and from thence the chain passes on to the next carriage. The two guide pulleys, and the pulley on the horizontal arm of the bell-crank lever, form, with the chain, an isosceles triangle, with its apex downwards.

The action of the brake is easily seen: as the chain is wound on the barrel, the "pulley" at the end of the bell-crank lever, and the lever with it, is raised; the vertical arm, moving away from the vertical, actuates the thrust rods, which in their turn actuate the levers carrying the brake blocks.

The device for enabling the guard to bring the surfaces of the van-axle friction roller and the chain-barrel friction drums into contact, requires a few words of explanation.

The swinging links carrying the latter are hung, one from the carriage frame near the fulcrum of the long lever; the other is hung from the lever, as seen in the large diagram. The lower ends of these swinging links are, by means of short links, hinged to the lower end of a short link, whose upper end hinges on a pin on the long lever.

When the guard wishes to apply the brake, he raises the lever by means of a hand-wheel and screw; the lever, in rising, draws up the short inclined links, which pull the "friction drums" into contact with the roller on the van axle.

This brake may be made instantaneous in its action by placing a spring underneath the lever, which is compressed, and then released by the guard when required for raising the lever and applying the brakes.

This brake is now out of date; it does not comply with the "Board of Trade" condition regarding automatic action in case of accident. The brake was much affected by frost and atmospheric changes, and was unreliable, as well as being useless if the train became parted. It was likewise rough in its action.

THE "SMITH" VACUUM BRAKE.

Each carriage carries one or more collapsible cylinders, whose lower ends are linked to the bell-crank levers of the brake rigging. A continuous train pipe sets up a through communication between an ejector on the engine and the collapsible cylinders throughout the train.

The collapsible cylinders are in *stable* equilibrium when the brakes are "off," being so maintained by a pressure of air within the cylinder, which is the same as that without the cylinder.

To apply the brakes, this equilibrium is destroyed in the following way: a jet of steam is blown through the ejector, which draws the air from the continuous train pipe and the cylinders it connects; these, being exhausted of air, collapse under the pressure of the external air, and apply the brakes.

This brake does not comply with the Board of Trade requirements in several respects.

(*a*) It is not instantaneous in action. It cannot be applied by the guard.

The brake is efficient in stopping trains so long as there is no leakage in the pipes or cylinders to neutralize the action of the ejector.

(*b*) In case of accidental parting of the train, or in the case of derailment, the brake is useless.

The brake does not command the driver's confidence, as it is unreliable. The brake is an example of Class III.

The "Hardy" Brake

Resembles Smith's. Two ejectors are used, one of which exhausts the cylinders on engine and tender, the other acts on the continuous train pipe, and exhausts the cylinders on the carriages. It would be more correct to describe the ejector as double, than as two. The cylinders differ somewhat from "Smith's"; their lower ends are open to the air, the piston fits loosely, and is made air-tight by means of an elastic diaphragm, the outer edge of which is bolted between the flanges of the cylinders, which are cast in two halves, the inner edge being fitted to the piston to exclude the air.

The action of the brake is identical with "Smith's." It is efficient in stopping trains while there is no leakage. In the event of the hose couplings parting, the brake on the carriages is rendered useless, though that on the engine remains unaffected. The "Hardy" brake is in this respect an improvement on Smith's.

THE "TELL TALE" VACUUM BRAKE

Resembles Smith's in the use of *one* ejector only, and Hardy's as regards the brake cylinder. The "ejector" differs from either; it contains within the large ejector a small auxiliary one, which maintains a certain degree of vacuum in the train-pipe to test its soundness; this amount of vacuum is indicated on a gauge by a pointer, showing "brake right"; should a leakage occur that would endanger the action of the brake, the small ejector will fail to maintain the vacuum, which the pointer will show on the gauge.

THE "WESTINGHOUSE" STRAIGHT AIR BRAKE.

This is another example of Class III. It is, however, an air-pressure brake.

An air-pump on the engine, worked by steam from the boiler, forces air into a large reservoir placed underneath the tender. The pressure in the reservoir varies, but is generally 70 to 80 lbs. on the square inch. Double piston brake-cylinders are fitted under the carriage frames, the piston-rod ends being connected with the brake-rigging by means of short connecting rods.

A continuous brake-pipe runs from the air-reservoir on the engine tender along the train, being connected by branch pipes with the brake-cylinders.

To apply the brakes, the driver opens a valve, which admits air from the reservoir to the "train-pipe"; the pressure in the pipe and the brake cylinders is thus raised till it equals that in the large reservoir, the brake-pistons in the cylinders are thrust apart, and the brakes applied.

This brake was effective in stopping trains, so long as no serious leak occurred in the pipe from bursting of a hose-pipe, or the unfastening of a pipe coupling.

The brake was furthermore much quicker in its action than any simple vacuum brake could be, on account of the high pressures used. It will be seen, however, that, as described, the brake power would be lost should a pipe coupling become undone. To mitigate this disadvantage, Mr. Westinghouse added a strong auxiliary reservoir of large capacity to the tail brake-van, in which air at high pressure was stored. Communication between this supplementary reservoir and the train pipe was cut off during ordinary running by a guard's valve.

Valves were likewise placed in the hose couplings, which, in the event of a coupling becoming undone, were pressed against their seats by the pressure within the pipe when communication was opened with the supplementary air reservoir by the guard's valve. It will thus be seen that, in the event of the train parting, the power of the brakes would not be entirely lost, the brake power being preserved to the part of the train connected with the rear brake-van. This property of retaining a portion of the brake-power in the event of parting of the train, the "Westinghouse" shares with the "Hardy" vacuum brake; but in the case of the "Westinghouse," the value of this power is enhanced because the brake power is preserved to the *rear* portion; the front part of the train connected with the engine is controlled by it, because the driver can command a certain amount of braking power, if he chooses to throw his engine in "reversed gear" while running forward; in any case, a driver can put on the tender hand-brakes.

The "Westinghouse" simple air brake is used on the Metropolitan Railway.

We now come to the second division, containing the Automatic Brakes of Class III.

B. AUTOMATIC CONTINUOUS BRAKES.

Clarke's brake belongs to Class I.

In this brake long tension rods,—one running on each side of the centre line of the train its whole length,—are connected with the principal levers of the brake-rigging, which they actuate. Between the ends of the principal levers and brackets bolted to the tie-beams of the carriage underframes, these tension rods are enveloped by long spiral springs, the function of which will be seen directly.

The tension rods are pulled by means of screw gearing in the guard's van, worked by the guard with a hand wheel.

When the train is about to start, the guard, by means of the screw-gear, pulls the tension rods, and compresses the spiral springs, the brakes being at the same time released.

It will be seen that all these springs, in compression, constitute a store of power, which can be used for applying the brakes when the tension rods are released by the guard, or which will automatically apply the brakes on both portions of the train, should it become parted. A device is also often added for instantaneously releasing all the springs by disconnecting the screw-gearing and the tension rods; when, of course, the brakes are applied throughout the train.

This brake complies with the Board of Trade requirements, as regards conditions *a* and *b*.

Its chief drawback is the use of powerful screw-gearing, which *must* be employed to enable the guard to store a serviceable amount of power in the springs. The mechanical advantage gained by the screw-gearing implies a waste of time in getting the brakes "off" and storing power in the springs for the next application of the brakes.

The next brake belongs to Class II. of the Automatic division. The brake is—

"Clark's Chain Brake."

The friction rollers and friction drum, whose axle carries the chain barrel, are shown in the large diagram.

The chain, after passing from the barrel on which it is wound, passes over a guide pulley, under another hung in a frame which slides on vertical guides, and then again up over a guide pulley, after which it passes on to the next carriage.

The bracket carrying the pulley *under* which the chain passes, is kept down by spiral springs enveloping the vertical rods which guide the pulley bracket.

This guide bracket carries a hook, which hooks over the pin passing through the ends of the links, whose other ends are attached to the swing frames carrying the guide pulleys over which the chain passes. It will be seen, that when the chain is wound up, the bracket carrying the guide pulley rises until the hook it carries can lay hold of the fulcrum pin of the thrust rods; in rising, the bracket compresses the spiral spring, which thus contains a store of power.

To apply the brakes, the chain is slackened, by which the springs, through the medium of the bracket and hook, exert a downward pull on the links, which thrust apart the suspension links carrying the chain pulleys; the pulling rods connected to the brake rigging are likewise attached to these suspension links. Thus, when the suspension links are thrust apart, the brakes are pulled on.

This brake has been extensively used. It complies with the Board of Trade requirements, when not affected by the weather. The friction pulleys occasionally become coated with ice, when the brake is rendered useless. Thus the brake is not quite reliable. The brake is quick in its action, but jerky, because the friction between the friction rollers will not admit of graduation.

It can only be used on short trains; on long trains the resistance of the springs exceeds the force that can be transmitted by friction gearing worked from the rear axle.

THE "HEBERLEIN" BRAKE.

This belongs to Class II. In "Heberlein's" system each carriage carries a complete set of friction pulleys and gearing, connected by tension rods with the brake rigging, as shown in Figs. 1 and 2 for the engine, and in Figs. 5 and 11 for the carriages.

The friction gearing is the most important part of the brake apparatus. A **Z**-shaped frame, shown in side and front elevation in Fig. 4, carries a friction pulley, the lower one of the two, and a chain pulley, the upper one; the chain pulley is placed to the side of the friction pulley; a stepped chain of peculiar construction, made of flat links, is wound off the upper pulley on to the barrel of the lower, or *vice versâ*, according as the brakes are in the act of going "on" or coming "off."

The rear axle of each carriage carries a friction pulley made in halves, this friction pulley being secured by a key and by bolts. When the brakes are "off," the **Z**-shaped frame is held *away* from the axle by the tension of the rope transmitted through the inclined tie-rod attached to the swing frame. When this is the case, there is no frictional contact between the pulley on the axle and that carried by the swing frame.

To apply the brakes, the driver or guard lets out sufficient of the cord from his "winding reel" to allow the **Z**-shaped frames to descend, and bring the surfaces of the two friction pulleys into contact. When this is the case, the "stepped chain" is wound off the chain pulley on to the barrel of the friction pulley; the short piece of flat-link chain is wound on to the excentric barrel of the upper chain pulley; and the horizontal tie-rod, with which the short piece of flat-linked chain is connected, pulls on the brakes.

The "Heberlein" brake complies with the Board of Trade requirements. It has, however, been very little used in England. It is extensively used on the German railways, and has also been fitted to a good deal of the rolling stock sent out to South America. The Bristol Wagon Works have fitted it to stock for Spanish and South American lines.

In this country, the brake is in use on the Colne Valley Railway; also on the Highgate Hill Cable Line. The brake gives satisfactory results; it is automatic in its action as well as instantaneous. The reels and tackle do not commend themselves to an English eye; and the brake, like other chain brakes, is open to the objection of being affected by the weather and by frost. Reports, however, speak well of the brake, which is, without doubt, the best of its kind. In some respects it resembles Wilkin & Clark's. This brake concludes the list of those to be given in this class.

We now come to the most important class of continuous brakes; those belonging to Class III., Automatic Division.

Among these are numbered the various types of automatic vacuum brakes in use, the different systems of air-pressure brakes, and the ingenious "Barker" hydraulic pressure brake, which in certain respects resembles the "Westinghouse" automatic brake and the "Hardy" automatic vacuum brake, to be presently described.

The brakes of Class III., whether "simple" or "automatic,"—but the latter more especially,—have to a very large extent displaced, and will eventually entirely supersede, all other kinds of brakes.

Two distinct methods are followed for giving motion to the brake-piston:—

1. That of "differential" pressures, in which the brake-piston, when the brakes are "off," is maintained in a condition of unstable equilibrium, and in which the normal unstable equilibrium is destroyed for application of the brakes.

2. That of using separate "vacuum" or "compressed air" reservoirs, or, in the case of Barker's hydraulic brake, a reservoir in which water is stored under pressure, from which reservoirs, in each instance, communication is opened with the brake cylinders when the brakes are to be applied. When the brakes are "off," the communication between the cylinders and reservoirs is closed, the brake piston being then held in *stable* equilibrium by air on each side at the normal atmospheric pressure outside the brake-cylinder, the brakes being released by their own weight, or by springs.

To the class of "*Differential*" *pressure* brakes belong the "Automatic" vacuum brake, the Steel & McInnes air-pressure brake, the "Clayton" automatic vacuum brake, the "Wenger" air-pressure brake, and "Aspinall's," "Smith's," and "Saunders's" automatic vacuum brakes.

To the class of "Separate Reservoir" brakes belong the "Hardy" automatic vacuum brake, the "Westinghouse" automatic air-pressure brake, the "Eames" vacuum automatic brake, and the "Barker" hydraulic automatic pressure brake.

Of the "Differential" pressure brakes, it will be sufficient to describe the "Automatic" vacuum brake, and "Steel & McInnes" air-pressure brake, as they are typical examples. Of the "Reservoir" brakes, the "Hardy" automatic vacuum, the "Westinghouse" automatic air-pressure, and the "Barker" automatic hydraulic pressure brake are the most interesting types.

"DIFFERENTIAL" PRESSURE.

The "Automatic" Vacuum Brake.

An "ejector" on the engine withdraws the air from the continuous train-pipe, and from both sides of the brake piston, working in a cylinder placed in an enlarged tub; the brake pistons are thus placed in unstable equilibrium, and the brake-blocks are "off." To apply the brakes, the driver puts the ejector handle to "Brake on," thereby admitting air to the train-pipe and lower sides of the brake-pistons, at the same time sealing the vacuum on the top side by means of the "ball" in the valve, which is pressed against its seat; the brake-piston is thus pushed up, and the brake applied.

This vacuum brake complies in every respect with the "Board of Trade" requirements, and is the best of the vacuum brakes.

Its defects are, that on account of the large volume of air which has to be withdrawn from the cylinders and train-pipe through the ejector, the brakes cannot be pulled "off" as quickly as is sometimes requisite.

The second defect is, that the same large amount of air has to be admitted to the train-pipe for application of the brakes, though in this case an automatic valve in the guard's van is available, in addition to the driver's valve; hence the brake cannot be got "on" nearly as quickly as where compressed air is used.

The application of the brakes might be hastened by applying "automatically" opening valves at various points in the train-pipe for admitting air; but the chances of something going wrong would be likewise increased.

This brake is very extensively used; and it gives the driver complete control over his train.

THE STEEL & McINNES BRAKE.

An air-compressing pump on the engine forces air along a continuous train-pipe to the top and bottom sides of the brake piston, working in a cylinder, with which each carriage is supplied. The lower part of these vertical brake cylinders is enlarged to form an air vessel. When the brake-piston is in *unstable* equilibrium, the brakes are "off."

To apply the brakes, air is discharged from the top side of the brake-piston, when the air below it and in the air vessel expands, and pushes up the piston, thus applying the brakes. The piston rod passes through the lower end of the vertical cylinder, and is there connected with the brake levers by a connecting rod. The piston rod passes through a stuffing box and gland, which, if not kept properly packed, gives rise to trouble in practice by reducing the pressure below the piston.

The McInnes brake complies with the Board of Trade requirements.

It is not used in England, but is in Scotland, though not extensively.

The enormous quantity of air which has to be discharged from the top ends of the cylinders and train-pipe when the brakes are to be applied, and then re-supplied when the brakes are to come "off," renders it slow in action.

On a carriage 30 feet long, with a brake cylinder 8 inches diameter, 12 inches stroke, the capacity of the cylinder being 600 cubic inches, and capacity of the air vessel 1,800 cubic inches, the train-pipe on each carriage measuring 284 cubic inches, and assuming the piston to travel 6 inches, 884 cubic inches must be discharged for a full application of the brakes.

Mr. Michael Reynolds, in his work on "Continuous Brakes," supplies these figures.

A quick air-pump of large capacity is necessary.

Class III. (Reservoir Type.)

THE "HARDY" AUTOMATIC VACUUM BRAKE.

In the "Hardy" brake an ejector on the engine exhausts the air from the train-pipe, the vacuum reservoir on the engine, and the vacuum reservoirs placed under each carriage. In addition to the vacuum reservoir, each carriage carries a brake cylinder, whose piston actuates the brake levers.

When the brakes are "off," communication is cut off between the brake cylinder and the vacuum reservoir by means of a double-seated diaphragm valve, which is maintained in a condition of unstable equilibrium by the vacuum in the pipe on one side and the vacuum in the reservoir on the other. The brake piston is maintained in *stable* equilibrium by a pressure on both sides, the same as that external to the brake cylinder, the brakes being held "off" by the weight of the pistons.

To apply the brake, the driver admits air to the train-pipe, thereby destroying the vacuum and the equilibrium of the diaphragm valve, which is thrown over, and opens a passage from the "reservoir" to the brake cylinder, the air on the top side of the brake piston expands into the vacuum reservoir, and the brakes are applied by the external atmospheric pressure on the lower side of the brake piston.

As to the merits of the "Hardy," it complies with Board of Trade requirements. The use of a vacuum reservoir of large dimensions, an absolute necessity, is a *drawback*, as is also the use of a diaphragm valve with double seat.

It will be seen that the movement of the valve depends on the pressure on the diaphragm; and the direction in which it moves the brake determines whether the brake is going "on" or "off."

The "Hardy" brake is heavy, and all the parts are large.

THE "WESTINGHOUSE" PRESSURE BRAKE.

In the "Westinghouse" automatic pressure brake, the use of a separate reservoir attains its best results, as the use of "differential" pressures does in the case of the automatic vacuum brake. The results obtained by the "Westinghouse" brake have not been equalled in any other system. The following is a brief description of the brake.

An air-compressing pump, worked with steam from the engine, forces air into the main reservoir on the engine. From the main reservoir a pipe leads by way of the driver's brake valve to the continuous train-pipe, which, by means of branch pipes, communicates through the "triple" valve with the auxiliary reservoirs under the carriages.

Each reservoir communicates with its brake cylinder also by way of the "triple" valve.

Supposing, now, the main reservoir to be stored, and the air-pump to be working slowly, and the brakes to be "off," the train-pipe and auxiliary reservoirs being all charged. The pressure in the main reservoir is about 15 lbs. higher than the pressure in the train-pipe. The object of this will be seen later on.

When the brakes are "off," the train-pipe pressure is equal to the reservoir pressure, the "triple" valve of each reservoir being held in *unstable* balance between the pressures.

When the brakes are "off," the brake-piston is in communication with the atmosphere, the piston being kept home, and the brakes "off," by the "spiral" springs.

To apply the brakes, the driver discharges air from the brake pipe, the equilibrium of the "triple" valves is thereby destroyed, they fall and open a communication between the auxiliary reservoirs and the brake-cylinders, the air from the reservoir forcing out the brake-pistons and applying the brakes.

When the brakes are to be released, the driver opens a communication between the main reservoir and the train-pipe, the excess of the air pressure in which admits of its supplying the train-pipe with the air necessary to restore the "triple" valve to its former state of unstable equilibrium. The "triple" valve, being thus raised, cuts off the communication between the auxiliary reservoirs and the brake-cylinders, and places the latter in communication with the atmosphere through the exhaust passage, thus allowing the brake pistons to be forced home by the springs, and the brakes to be released. When the triple valve reaches the groove at the end of its travel, air again feeds past it into the auxiliary reservoirs, until the normal pressure in them is again reached.

In a Paper on Brakes, read before the Society of Arts by Mr. W. P. Marshall, M.I.C.E., he stated that the "Westinghouse" brake could not, like the Automatic Vacuum Brake and other Differential Pressure brakes, be applied with any desired degree of pressure, and be left for any length of time at that pressure. That this statement is misleading and erroneous, the author has had opportunities of proving.

Now, as to the merits of the "Westinghouse" brake.

(*a*) It is efficient in stopping trains, is absolutely reliable, and easily applied by driver or guards. It is automatic in its action.

(*b*) Owing to the use of a large reservoir carrying a considerably higher pressure than the train-pipe, and by the use of auxiliary reservoirs and high pressure, the brakes can be got "on" and "off" faster than any other brake.

The time taken to apply the "Westinghouse" brake is about three seconds on a train 400 feet long; the time taken to get the Automatic Vacuum brake on is about seven seconds at the best.

The "triple" valve has been criticized in the past on account of its complication. In spite of its so-called complication,—which is not more than that of an ordinary piston and steam cylinder,—it does not fail as the valves of the vacuum brakes do, which are always causing delay. The Board of Trade returns record few cases of a "triple" valve failing to work, though there are a great number of failures recorded against various types of vacuum brakes.

The "vacuum brakes" may show a few more miles run for a "failure" than the "Westinghouse." The failures of the vacuum brakes are such as would, under certain conditions, fail to prevent, and in some instances have been productive of, accidents.

The failures recorded against the "Westinghouse" are in no instance dangerous failures. The worst they have done, is to pull a train up by the accidental bursting of a hose coupling—a mishap that cannot result in a disaster where the block-system, or other system of signalling trains, is properly worked.

Comparing now the "Westinghouse" automatic pressure brake with the "Steel & McInnes" pressure automatic brake, and assuming the same diameter of cylinder, 8 inches, the same maximum piston travel 6 cubic inches, and the auxiliary reservoir 1,800 cubic inches capacity, and the train-pipe capacity to each carriage 284 cubic inches, the volume swept through by the brake-piston when the brakes are full on being 300 cubic inches, we shall find that, whereas it was necessary for a full application of the brake (differential pressure) to discharge 884 cubic inches of air on each coach, it is only necessary to discharge 40 cubic inches in the "Westinghouse" brake to fully apply the brakes. It is evident that the "Westinghouse" can be applied faster, taken "off" faster, and that it is more economical in the use of air.

Though the amounts of air which have to be discharged in the "Steel & McInnes" and the "Westinghouse" are for a full application of the brakes, the actual amounts used in working the brakes are only as 2½ to 1.

The next brake is Barker's Hydraulic Brake.

Barker's Hydraulic Brake.

In Barker's brake a force-pump on the engine forces water along a continuous brake-pipe to an "accumulator," placed on each coach. The water is under the pressure of a very powerful coiled spring, placed under compression by a piston which the entering water forces forward.

A slide valve, carried in the piston in the auxiliary piston-valve, is placed in a condition of unstable equilibrium by the pressure of water in the train-pipe on one side, acting against a coiled spring on the piston-valve rod, as seen in the wall diagram.

In certain positions the auxiliary piston valve opens a communication between the accumulator and the brake cylinders. This occurs when the train-pipe becomes broken, the coil spring, then unbalanced, forces over the valve 1, and opens a passage from the accumulator to the brake cylinder through the port 2, the brakes being applied. This brake is not used; it is an example of an able and ingenious endeavour to cope with the difficulties that beset the practical solution of the brake-question.

The principle of the brake closely resembles "Westinghouse's." The trial brakes answered well; but the use of water barred the way to the general adoption of Barker's hydraulic automatic brake.

There is one point which has not been referred to in dealing with air and vacuum brakes of the automatic division.

This is the "leak off" device, adopted in the "Clayton and Smith" brakes, for allowing the brake to come "off" after it has been "on" from 80 seconds to 120 seconds.

It will be seen from the principle of the automatic brakes that the brakes go "on" when a hose coupling becomes undone, or when air is admitted to the train-pipe for application of the brakes. Now, in the "Automatic Vacuum" brakes, where there is no "leak off" hole in the piston, the brakes will remain "on" until released by the "ejector" withdrawing the air from the train-pipe, or until the driver allows air to flow to the top as well as to the bottom side of the brake piston, by means of the release valve and lever attached to each cylinder. To release, the guard pulls the cord connecting the release valve levers, which pulls the ball away from its seat, allowing the air to flow to the top side of the piston, thus balancing it and releasing the brakes.

Now this release valve device is only provided to admit of the carriages being moved when the engine has left the train at a station, or for shunting purposes.

The "leak off" device,—which is nothing less than a death trap,—allows the brakes, as before said, to come "off" by themselves, after being applied for from 80 to 120 seconds. The dangerous nature of such a device has been often shown when trains have crashed into "buffer stops," owing to the brakes leaking "off," the driver meanwhile believing he had applied the brakes at the right time. The "leak off" device makes the brake quite unreliable. In the event of a train parting when going up a steep bank, the train will be pulled up for a minute, and then allowed to run backwards to the bottom of the bank. In short, it renders the protection conferred by fulfilment of the automatic condition, when really adhered to, futile.

The "Westinghouse" brake is, like the automatic vacuum brake, provided with a release valve; but neither of these, the two best brakes in use, are provided with such dangerous devices as "leak off" holes.

SUMMARY OF CONCLUSIONS

With regard to the use of continuous brakes is—

1. That the use of continuous screw brake and other continuous hand brakes, like Clark's, is declining, and almost ended.

2. That "chain brakes" are likewise going out, on account of their liability to be affected by the weather, and their rough action. If some device can be invented which will render them reliable in all weathers, they may yet find their place on mineral trains and goods trains, which are at present ill supplied with brake-power.

3. That by the use of automatic air pressure, or of vacuum brakes, the speed of trains is most effectually con-

trolled, and that these brakes are destined to displace all others.

The tendency in favour of "high pressures" and light parts, and other attendant advantages, points to "pressure" brakes as the best means of solving, in a practical manner, the brake-question.

The following are the further conclusions at which one arrives after examining the various types of vacuum and pressure brakes:—

1. That where, as in the case of vacuum brakes, the total force which applies the brakes depends upon the degree of exhaustion on one side of the piston, better results are obtained, and a more perfect vacuum, with a "differential" pressure brake, such as the "automatic" vacuum brake, than is obtainable in a reservoir brake like "Hardy's."

2. That, on account of the large amount of air which has to be passed through a limited number of valves, "differential" pressure brakes, whether vaccum or high pressure, must always be much slower in going "on" and coming "off" than brakes of the separate reservoir type.

3. That "separate reservoirs" are most suitable for air pressure and hydraulic automatic pressure brakes.

4. That the pressure principle, as exemplified by the "Westinghouse" brake, is the only one which has yet solved the brake question to its fullest extent for trains of extreme length 2,000 feet.

5. That brakes in which a "leak off" hole is provided, to allow the brake to come "off" without the application of the "ejector," can only be regarded as nominally complying with the automatic condition.

Future Engineering.

By HARWOOD H. SIMPSON.

(*Abstract of Paper read April* 17*th*, 1888.)

FUTURE engineering depends upon certain requirements of the human race, and upon the carrying out of the works necessary for supplying those requirements. First is placed the transmission of the senses or functions of seeing, hearing, feeling, etc., quickly, almost instantaneously, throughout the space of the earth, across both land and water. There is every reason to believe that this will be done by means of electricity, and its accomplishment would be an immense benefit to humanity. Signs are already transmitted in this manner by the electric telegraph; and, owing to late progress, speaking has been carried on between places five hundred miles apart. Hitherto electrical engineering has been a small branch of engineering; it is now a most important branch, and will probably in the future be the most important. At present it employs a capital of at least £250,000,000, an enormous staff of servants of all grades, and demands the production of special materials and machinery; and when we think of the extension of this field (including the production, utilization, and transmission of light, heat, and power, to which reference will be made), which new discoveries and improvements will cause, there is every reason to look with hope to the future of electrical engineering.

The next great requirement is the transmission of solid bodies at the greatest possible speed through space, both on land and water. As greater speed can be obtained on land, endeavours will be made to utilize it to the greatest extent. In either case it will be essential to choose the shortest possible routes, and this means—by land, the bridging of seas, rivers, and valleys, and the tunnelling of seas, rivers, and mountains—and by water, the connecting seas, rivers, lakes, and other waterways by means of canals. For the transmission of heavy weights, when cheapness of carriage, and not speed, is the greatest object in view, canals are likely to be made. One great advantage of these works will be the saving of time and labour through not requiring to break bulk. It will be seen that these engineering works of the future will be of great magnitude and cost, compensating in this way for the large quantity of less expensive work of the past. Works of this nature have already been completed, such being the Suez Canal, the Mont Cenis and the St. Gothard Tunnels, the Tay Bridge, the Brooklyn Bridge, and the Severn Tunnel, whilst there are in progress the Forth Bridge, the Corinth Canal, the Panama Canal, and the Manchester Ship Canal. In the case of railways across waterways, the question as to whether bridging or tunnelling is best will probably not be settled until several such works are completed. In the case of tunnels being made, there is little doubt that electric locomotives will be used in the future. Two great works which are likely to be carried out in the near future are the joining of England and France, and also of Scotland and Ireland, by means of tunnels or bridges. The completion of railways to India is a question likely to be settled, seeing that the journey from England could be performed in seven days, or one quarter of the time it now takes by water. There are several great schemes being advanced for making canals, viz., Mr. Keeling's canal for connecting Birmingham and the Bristol Channel; a canal from Sheffield to the Humber; Mr. Lloyd's scheme proposes to make Birmingham the centre of a large system of canals, with branches to London, the Bristol Channel, the Humber, and the Mersey. The construction of these canals will lead to great improvements in our natural waterways. In these future schemes one can foresee a great field for engineers in all branches; their carrying out will require much thought and invention, and their progress should be watched with interest by young engineers especially, who should study their requirements in order to bring to bear upon the practical work their knowledge and assistance.

Next in order of requirement come the supplying to communities, both large and small, of water, light, heat, and power. The extreme dryness of last summer proved our water supply to be quite insufficient in a great many places. Our present supply consists of surface water—containing impurities of all kinds, and requiring to be filtered—and water which has been naturally filtered through various depths of the earth, and is ready for use. Engineers are likely to be called upon to substitute the latter supply for the former; and this means the reconstruction, at a probably early date, of a large number of waterworks. London is almost wholly supplied with water taken from the surface, the quantity taken being nearly one-third of the quantity available. Artificial filtering is liable to failure, owing to withholding the expense and labour necessary, and it is likely to be given up in the future. The carriage of water, by means of pipes, is so easy that we may look forward to its extension all over the country, and it is to be hoped also to the adoption of subways for holding the mains, as also gas, electric, and other mains, which will abolish the senseless custom of constantly breaking up our roads. In supplying water in dry countries like Australia and South Africa, there is great scope for engineers, as want of water is the chief drawback to those countries. One reason why small works of this description have not been more largely carried out in this country, has been the difficulty and expense of obtaining an Act of Parliament; but under the proposed local government there will be greater facilities, and there seems a prospect of greater activity in this direction for engineers.

With regard to the future of lighting and heating, the electric light is likely to be the light of the future; and the oils, fats, and gases now used for lighting will probably be more advantageously used for heating. The electric light is making slow but sure progress, the chief reason for its slow progress being the want of thoroughness and care in working, rather than to any defect of the light. Action is now being taken to make its use compulsory in public buildings. Gas is the chief opponent of the electric light; but up to the present the latter has had no effect in diminishing the consumption of gas, owing to the extended use of gas for heating. Heat produced by electricity is now being used for welding metals, and is said to be the most perfect and cheapest way. Lighting and heating are branches of engineering under-going great changes, perhaps more abroad than at home; and the study of these changes is necessary in this country.

The supplying and transmitting of power to communities may be said to belong wholly to the future. The powers most available are steam, hydraulic power, compressed air, and electricity. At present these powers are obtained by the consumption of fuel, chiefly coal; but the question of utilizing natural powers for their production, such as wind, tide, and waterfalls, is part of the future work for engineers. The transmission of power by steam is limited in distance, by loss of heat and condensation, to about three-quarters of a mile. The drawbacks of hydraulic power are the great consumption of fuel for the work done, and the great loss of power through friction in the pipes as the distance increases. Compressed air is very difficult to carry for a distance at a large power. Little progress has been made in this branch of engineering; but there is now a possibility of electricity being the means of accomplishing all that is desired in this direction. Fifty-horse power has been transmitted seventy miles with a loss of only 30 per cent. by this means.

In works of sanitation, comprising the collection, removal, and utilization of all kinds of refuse, there is much to be done in the future. The principle which appears to be the right one, is to utilize this refuse in enriching and improving the land. One question having a great bearing upon future engineering, is the production of wealth from the land progressing and keeping pace with the progress of other industries. The utilization of refuse on the land would greatly increase its production, and also to a large extent free our rivers from pollution. Sewage works carried out on this principle are amongst the most successful of the great number of different systems.

The future of those branches of engineering dealing with the production of materials, machinery, and tools, depends more upon the carrying out of the probable great works of the future, than upon the requirements of the works already carried out; and producers should think twice before opposing these works. Almost as bad as opposing them is withholding support. The Manchester Ship Canal was nearly killed in this way; and Bristol's support to that scheme was practically *nil.* Yet several Bristol firms are now supplying materials, machinery, and tools for that great work. There are locomotives at work on the canal which have been made in Bristol; and the best authority tells me that for materials, workmanship, and working power they are second to none in the great number by various makers there. There has been a tendency of late years to hasten trade by sending out bad goods, which instead of decaying by fair wear and tear, quickly come to an end owing to bad material and workmanship. To invent and produce something better than its predecessor, thereby causing a constant change and improvement, bringing with it increased business to both producer and consumer, should be the aim in future. It is when the latter way ceases to be used that the former becomes necessary. Englishmen have in modern times been the leaders in engineering, and they must endeavour to keep the lead; but this can only be done by their superior knowledge and skill, for acquiring which increased facilities must be provided in the way of instruction, both theoretical and practical, on the principle carried out by the College in which we are assembled.

A discussion upon the subject of the paper took place, in which Messrs. Morgans, McCurrich, Sutcliffe, Cotterell, and Harvey took part.

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